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To my parents

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An Empirical Assessment of the 2004 EU Merger Policy Reform*

Tomaso Duso[†], Klaus Gugler[‡] and Florian Szücs[§]

Abstract Based on a database of 326 merger cases scrutinized by the European Commission (EC) between 1990 and 2007, we evaluate the economic impact of the change in European merger legislation in 2004. We first propose a general framework to assess merger policy effectiveness, which is based on standard oligopoly theory and makes use of stock market reactions as an external assessment of the merger and the merger control decision. We then focus on four different dimensions of effectiveness: 1) legal certainty; 2) frequency and determinants of type I and type II errors; 3) rent-reversion achieved by different merger policy tools; and 4) deterrence of anti-competitive mergers. To infer the economic impact of the merger policy reform, we compare the results of our four tests before and after its introduction. Our results suggest that the new approach of the EC, which is more firmly anchored on economic principles, resulted in a better identification of problematic cases, a reduction of type II errors, and a slight increase in remedies effectiveness. However, the predictability of outright clearances has initially decreased. Finally, the policy shift away from prohibitions that was reinforced after the introduction of the reform does not seem to be well grounded.

Keywords: merger control, regulatory reform, EU Commission, event-study

JEL Codes: L4, K21, C13, D78

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Introduction

By reforming the present merger control system as radically as needed, therefore, I am determined to ensure that it remains a key instrument to foster Europe's economic success in the years ahead.

Mario Monti, EU Commissioner for Competition, 7 November 2002

The modernization package of European merger control, initiated in 1999, led to the adoption of Council Regulation 139/2004 in May 2004 (ECMR 04). Several observers interpreted this major institutional change as a shock reaction to events that had happened in the early 2000s, when three prohibition decisions of the Directorate General for Competition (DG Comp) were overruled by the Court of First Instance (CFI).¹ In all three successful appeals, the CFI identified the main problems as being related to the rigor of economic analysis conducted by DG Comp and the standard of proof the decision was based upon. While these reverses certainly were an indicator of the need for reform, they were not the cause: A Green Paper calling for a revision of European merger law had been published as early as December 2001.

One of the major goals of the merger policy reform was to achieve what became known as a 'more economic approach' in merger control, i.e. an approach closer to economic principles. Numerous important changes were made along these lines: an efficiency defense clause was introduced, the office of the chief economist and his team were created, the timetable for remedies was improved, guidelines for horizontal mergers were issued, and the old 'dominance test' (DT) was abandoned in favor of the 'significant impediment of effective competition test' (SIEC).² This last point is probably the most substantive change introduced by the reform. The main problem with the old DT was that it worked as a cumulative two-part test. A merger was to be declared incompatible with the common market if it '*creates or strengthens a dominant*

¹The cases in question are *Airtours/First Choice*, *Schneider/Legrand* and *Tetra Laval/Sidel*.

²Lyons (2004) discusses these reforms in greater detail. The problems with the DT and the advantages of the SIEC are summed up in Vickers (2004).

position as a result of which effective competition would be significantly impeded'. This implies that – as was later confirmed by the CFI – the second part of the test, the impediment of effective competition, only applied if the criterion of the first part, the creation of a dominant position, was met. Mergers reducing effective competition without the creation of a dominant position could not be challenged under the old legislation, whereas the creation of a dominant position is no longer a necessary condition for intervention by DG Comp post-reform.

The reception of the ECMR 04 was generally favorable. Commentators expected the quality and effectiveness of the decisions to improve due to the new approach, which enables a more flexible analysis closer to economic principles. Yet, some commentators feared that the cost of increased flexibility stemming from the adoption of more sophisticated tools in the assessment of unilateral effects and efficiency gains could be a loss in predictability of the merger control process.

Several years have passed since the introduction of the new merger regulation, enough time to make the first assessment of its effects. In this paper, we propose a comprehensive approach to empirically evaluate whether the modernization of European merger control has succeeded in attaining the goal of increasing its effectiveness. We analyze 326 mergers covering most major cases scrutinized by DG Comp until December 2007 to empirically assess the *economic impact* of the change in legislation and institutions brought about by the new ECMR 04. We base our evaluation exercise on a number of maintained theoretical assumptions coming from standard merger theory in an oligopolistic setting (e.g. Farrell and Shapiro (1990)) and the use of stock-market event studies to measure the effect of mergers and merger control decisions. From this starting point, we propose four dimensions of effectiveness of EU merger policy: predictability, decision errors, rent-reversion, and deterrence. For each of these, we adopt a before-and-after approach to single out the effects of the reform.

First, we test the predictability of the European merger control procedure. We estimate a simple probit model, where the decisions of DG Comp are a

function of observable characteristics. Consistent with previous studies (e.g. Aktas, de Bodt, and Roll (2007), Duso, Neven, and Röller (2007), and Duso, Gugler, and Yurtoglu (2011)), we find that several institutional and procedural variables play a significant role in explaining the Commission’s decision-making. We find evidence that the ability to predict interventions has not been affected by the introduction of the reform, while the ability to predict an outright clearance has decreased.

Next we assess whether the introduction of the new merger regulation has influenced the frequency and determinants of systematic mistakes made by the EU Commission (EC).³ Following Duso, Neven, and Röller (2007), we initially assess the competitive consequences of the mergers using the abnormal stock market returns of competitors to the merging firms. We define cases in which DG Comp remedied a merger that the stock market regarded as pro-competitive (type I errors) as well as instances in which the EC failed to remedy mergers that were regarded as anti-competitive (type II errors). We observe that the overall frequency of errors did not significantly change after the reform. Yet, the EC’s more pro-active attitude after the reform produced a shift toward more type I and fewer type II errors. In phase 2, however, it appears that the EC has become too lenient, since type II errors increase during this investigation phase compared to pre-reform.

In a third step, we estimate the degree of rent-reversion induced by the different merger control instruments used by DG Comp. Under a set of maintained assumptions, the relation between the abnormal returns around the announcement of a merger and those around the EC’s decision can be interpreted to indicate the success of merger policy in eliminating anti-competitive rents created by a merger (see Duso, Gugler, and Yurtoglu (2011)). While we find statistically significant full rent-reversion for prohibitions pre-reform, we do not find such reversion for remedies prior to ECMR 04. We cannot estimate the effects of prohibitions after the reform since there are so few, however, we find weak evidence that remedies have become more effective.

³The terms DG Comp and European Commission (or EC) will be used interchangeably throughout this paper.

Finally, we look at a fourth dimension of effectiveness, which is related to the indirect or deterrent effects of merger control.⁴ An effective competition policy should induce firms to obey antitrust rules and, hence, deter firms from proposing anti-competitive mergers. Thus, we estimate the probability of a merger to be anti-competitive as a function of past EC decisions. This is a novel approach and adds to the (limited) existing literature that has only looked at whether merger policy tools affect the number of notified mergers (Seldeslachts, Clougherty, and Barros (2009)) or the proportion of horizontal to total mergers (Clougherty and Seldeslachts (2010)). We find that, pre-reform, prohibitions as well as phase-1 remedies have a deterrence effect on the likelihood of proposing an anti-competitive merger. Post-reform, the deterrence properties of blockings is replaced by the deterrence effects of withdrawn or aborted mergers. This finding might be explained by the policy shift away from prohibitions. This process, whose beginning does not necessarily coincide with the introduction of ECMR 04 has continued after the reform following the idea that remedies are a superior policy instrument to blockings.

Combining event study methodology with econometrics – an approach pioneered by Ellert (1976) – has proven to be a fruitful empirical methodology in the assessment of business combinations and merger policy.⁵ Whereas traditional techniques rely on indirect measures of market power such as concentration ratios or subjective measures of the importance of entry barriers and the prospect for coordinated effects, the event study approach allows us to directly compute an independent evaluation of the merger and the merger control decision.⁶ However, the legitimacy of the event study approach in evaluating mergers has been put into question since it itself presents several shortcomings.

⁴As pointed out by Sørsgard (2009), an optimal merger policy entails deterrence, i.e. the effect a decision has on firms' future merger behavior.

⁵The event study analysis of mergers was first extended to rivals by Eckbo (1983) and Stillman (1983). Una and Feinberg (2000), Aktas, De Bodt, and Roll (2004), Aktas, de Bodt, and Roll (2007), Duso, Neven, and Röller (2007), and Duso, Gugler, and Yurtoglu (2011) use this methodology to evaluate EU merger control.

⁶Monti (2008) discusses how stock market reactions could be incorporated into the EC's decisions.

Criticisms predominantly include the role of expectations and externalities in stock market data (e.g. McAfee (1988) and Fridolfsson and Stennek (2010)). We recognize the validity of these criticisms and propose several ways to deal with them. First, our sample yields a particularly accurate assessment of the rivals' identity, since Commission experts have carefully identified the relevant product market for every merger. We therefore reduce the potential bias toward zero of the abnormal returns earned by rivals as a group, which would be caused by including firms that are not fundamentally affected by the merger. Second, the merger's announcements and the Commission's decisions might reveal information other than the pure competitive or profitability effect of the event, such as the effect of industry shocks triggering a merger wave, future acquisition probability, and the information about the allocation of the roles of insiders and outsiders. We tackle these issues twofold. We carefully choose the announcement date and the appropriate event window to reduce the influence of other triggering shocks and, even more importantly, we correct for the expectations of market participants about the merger proceedings prior to events. By conditioning on the merger-specific information available around the merger announcement, our correction for the market expectations should help us insulate the pure surprise element for any specific event and, hence, help to measure the competitive effect of the merger and the merger control decision. Finally, we conduct a comprehensive series of robustness tests to derive consistent evidence.

The paper proceeds as follows. Section 2 is concerned with our basic framework, the methodology and main assumptions. Section 3 presents the sources of the data, some summary statistics, and the estimations of the merger and merger control decision effects by means of stock-market event studies. Section 4 presents the results of the probability of intervention estimation, the analysis of the frequency and determinants of type I and type II errors, the rent-reversion regressions, and the deterrence regressions respectively. Section 5 concludes.

Methodology

This section provides a unified framework for assessing merger control. This framework is then used to discuss our four dimensions of effectiveness via empirical tests, which have been partially developed in our previous work (e.g. Duso, Neven, and Röller (2007) and Duso, Gugler, and Yurtoglu (2011)) and are partially newly designed in this paper. The objective of our analysis is to use this general framework to measure the impact of the modernization package of European merger control by comparing the periods pre-reform (January 1990 to May 2004) and post-reform (June 2004 to the end of 2007).⁷

The starting point of our methodology is that merger control aims at avoiding anti-competitive (i.e. consumer welfare decreasing) mergers by either blocking or remedying them or by deterring them. One of the main challenges in the empirical assessment of merger control is the ability to, first, define and, second, measure the anti-competitive nature of a merger. Next, we clearly state the assumptions needed to address these identification and quantification issues.

Assumptions

Theoretical Identification

We define an anti-competitive merger as one that reduces consumer welfare.⁸ Our basic setting is a standard static merger model in oligopolistic markets. The well-documented result of this literature is that mergers exert two externalities on rivals. The *market power effect* captures the impact of the reduction in competition brought about by a combination, absent any efficiency gains

⁷We chose the date in which the new merger regulation legally came into force to define the pre- and post- reform periods. However in section , we discuss this issue and the robustness of our results to the choice of a different date.

⁸In this paper we assume that the antitrust agency has a consumer-welfare standard, which is the standard adopted by the European Commission as well as most other competition authorities. Thus, we will not discuss the "right" welfare criterion in merger control. For such a discussion see Motta (2004) and Neven and Röller (2005).

(Stigler (1950)). For instance, in a Cournot setting, when a subset of firms in the market merges and jointly maximizes profits, they find it optimal to reduce their production. Under mild assumptions, this effect triggers the response of the remaining market participants to increase their production but by less than the merging firms. Hence, aggregate market output in the post-merger situation decreases, price rises, and consumer welfare is lower (Farrell and Shapiro (1990)). A similar mechanism generates price increases of merging firms and rivals and a reduction of consumer welfare in models where firms compete in prices and goods are differentiated (Deneckere and Davidson (1985)). Hence, a horizontal merger creates a positive externality for the competitors of the merging firms: via the "price umbrella" it increases their profits. The second externality, called the *efficiency effect* (Williamson (1968)), relies on the assumption of merger-specific synergies: Economies of scale, knowledge sharing, patent-pooling, etc., allow the merged entity to produce more efficiently than before, increasing the competitive pressure on its rivals and thus exerting a negative externality on them.

In most mergers both effects co-exist and what matters for welfare is the net effect of these antipodal forces. As Farrell and Shapiro (1990) show, there exists a critical level of efficiency gains such that the market power effect is exactly compensated for and the new equilibrium price and aggregate production is the same pre- and post-merger.⁹ Looking at this net effect thus allows us to infer the competitive nature of a merger. When the positive externalities exceed the negative externalities, i.e. the efficiency gains are not enough to compensate for the market power effect, rivals' profits increase, while consumer surplus decreases, since prices are higher than before the merger. The first identifying assumption of our framework is, therefore, that a post-merger increase in competitors' profits is an indication of the merger being anti-competitive.

This identification assumption is quite general and robust and holds for a wide class of oligopoly models. However, it could prove problematic in some circumstances such as vertical or conglomerate mergers and mergers in a dy-

⁹Farrell and Shapiro (1990) show that these efficiency gains have to be rather substantial to outweigh the market power effect.

dynamic context. Vertical mergers may cause market foreclosure, where both rivals and, potentially, consumers lose depending on the parametrization of the demand function, thereby violating our identification assumption concerning the nature of the merger. In a more general dynamic model, where horizontal merger proposals are endogenous and come over time and an antitrust authority can set its optimal policy (Nocke and Whinston (2010)), the holding of our assumption depends on the nature of the sequence of mergers and the merger policy. We therefore control for the merger wave and the horizontal nature of the merger in our regressions. As a further robustness check that we discuss in section , we exclude from our sample mergers that are not purely horizontal and obtain qualitatively similar results as for the whole sample.

Empirical Measurement

The next step is to measure the profitability effects brought about by the merger and merger control decision. Following an extensive literature, we do that by using stock market reactions to the merger's and decision's announcements, i.e. a stock-market event study.¹⁰ This methodology relies on the semi-strong version of the efficient capital market hypothesis, which asserts that stock prices fully reflect the information available to the market on the given commodity at any point in time. This implies that it is not possible to outperform the market index using commonly available information, or, more formally, that the daily return of a commodity i ($R_{i,t}$) is proportional to the market index ($R_{market,t}$) at any given point in time t :

$$R_{i,t} = \alpha + \beta R_{market,t} + \varepsilon_{i,t} \quad (1)$$

where $\varepsilon_{i,t}$ is an i.i.d. error term. The idea that markets are informationally efficient is central to the entire event-study literature starting from Fama (1970) and constitutes our second crucial assumption.

¹⁰As reported by Kothari and Warner (2007), by the end of 2006, there were more than 500 published papers utilizing the event study methodology in different areas of economics.

Under this assumption, model (1) can be used to estimate the 'normal' return of a firm at any given point in time as $\hat{R}_{i,t} = \hat{\alpha} + \hat{\beta}R_{market,t}$. When observing a stock market reaction to the announcement of a particular event (e.g. a merger or a merger control decision), the change in the equity value (with respect to the 'normal' value) of firms affected by this event can then be taken as a measure of the (discounted) additional profits that are expected to accrue as a consequence of the event. This stock reaction, also called abnormal return, can be seen as a measure of the profitability of such an event and can be measured as $AR_{i,t} = R_{i,t} - \hat{R}_{i,t}$. Since there might be information leakages, which influence firm i 's return before (or after) the merger announcement, the total valuation effect of the event is defined as the sum of the daily abnormal returns within a window of several days around the event: the cumulative abnormal return (CAR). Finally, we aggregate these measures to obtain a profitability measure for the merging firms and the competitors by taking a weighted sum of the individual CARs, where the weights are represented by the relative market value of each firm. We call these measures "cumulative aggregate abnormal returns" (CAARs).¹¹

The measured CAARs around a merger's announcement (or the EC's decision) also might entail effects other than the pure competitive effects and, in particular, the effects of specific forces triggering the merger (e.g. Jovanovic and Rousseau (2002)), information about the roles of merging firms and rivals (e.g. Fridolfsson and Stennek (2010)), and the market expectations about the outcome of the merger control decision (Eckbo (1992)). Hence, the third important assumption of our methodology is that we can effectively control for the merger's triggering events and the allocation of roles, by choosing the right announcement dates and event windows. We use the date of the first merger-specific rumors in the business press as the merger announcement (e.g. Banerjee and Eckard (1998)). The surprise element to the stock market is likely to be largest around this date, since the likelihood that the merger is already anticipated is still low. Moreover, using the *merger-specific* rumors coupled

¹¹In the appendix, we provide a formal derivation and a discussion of the CAARs.

with a large event window should help us to control for the uncertainty in the allocation of the roles (acquirer, target, rival). In particular, we consider different event windows for the various events to account for different information leakages. For the merger announcement, we use a long window of 50 trading days before the event's date and 5 days after.¹² For the phase 1 decision, we use a short window of 11 days (-5, +5), since information leakages are likely to be modest before the phase 1 decision given the strict timing procedure of the EU merger control. For a phase 2 decision, however, we again use the long window of 56 days (-50, +5) to account for information leakages due to the investigation and negotiation process during that phase (see also Appendix).

Finally, to tackle the issue of market expectations about the merger proceedings, we estimate the probability of intervention and use it to correct our CAAR measures. The logic of this correction is as follows: The stock market builds expectations on the likely outcome of the antitrust procedure, which should already be priced in the stocks of merging firms and rivals at the announcement of a merger. Thus, neither at the announcement nor at the decision we do measure the whole extent of the rents generated by the merger or the whole effect of the EC's decision, but only an update of the market's beliefs. We thus have to adjust the measured abnormal returns for the expectations of the stock market. An (extreme) example of a prohibition might clarify the intuition. If we measure a rent of 100 million US dollars around the merger announcement, but the ex ante expectation of the market is that the EC will block this merger with a probability of 20%, the full extent of rents is actually $(100/(1 - 0.2)) = US\$125$ million. See Appendix for a detailed formal derivation. Given our setup, we are then confident that the corrected CAARs around merger j 's announcement A (Π_{fj}^{A*}) can be seen as a meaningful measure of the competitive effects of the merger on merging firms ($f = M$)

¹²Duso, Gugler, and Yurtoglu (2010), with a sub-sample of our data, show that the cumulative abnormal returns calculated using this large window correlate positively and significantly with an alternative ex-post measure of profitability based on accounting data. The correlation is, instead, much lower and, in the case of rivals, even negative when using shorter event windows.

or competitors ($f = C$), while the corrected CAARs around the EC's decision (D) on merger j (Π_{fj}^{D*}) can be seen as a meaningful measure of the effect of the decision on the group f 's profitability.

A final assumption, which is however only needed in the rent-reversion test, is that the market power and efficiency effects of a merger can, at least partially, be separated by an effective antitrust action: Well-implemented remedies imposed by the EC should eliminate the market power effect while preserving the efficiency gains generated by the merger. All of these assumptions as well as the consequences of their failure are discussed in length and justified in greater detail in Duso, Gugler, and Yurtoglu (2011).

Assessing Policy Effectiveness

The innovation and main contribution of this paper is to assess the economic impact of the introduction of ECMR 04. To achieve this goal, we look at four different dimensions of effectiveness and explicitly analyze the differences in the performance of the EC before and after the 2004 reform. These dimensions of policy effectiveness can be seen in a natural chronological order. First, before the announcement of a merger, legal certainty and predictability of the merger control procedure are important determinants of firms' choices on the kind of merger they propose and, hence, total welfare. Therefore, our first test analyzes the determinants of interventions by DG Comp to infer its predictability. The second event we look at is the EC decision. An effective policy should reduce mistakes; thus, we analyze the frequency and determinants of type I and type II errors committed by the EC. Third, it is not only important whether the EC intervenes in the 'right' mergers, but also whether its intervention achieves the desired results. Thus, we look at effectiveness as measured by the degree of rent-reversion achieved by the different merger policy instruments. Finally, a particular decision might have consequences on the future merger behavior of other firms. We therefore analyze the deterrence effects of the EC's decisions by estimating how past interventions affect the competitive nature of currently proposed mergers.

If the reform was successful in making merger policy more effective, we would expect (1) an increase in the predictability of the merger control outcome, (2) a reduction in the frequency of mistakes (decision errors), (3) a larger degree of rent-reversion achieved by remedies, and finally, (4) a higher degree of deterrence of anti-competitive mergers.

Legal Certainty, Transparency, and Predictability

The predictability of the merger control procedures is a key issue for judges, competition lawyers, authorities and, of course, the firms. Since legal certainty and transparency of the proceedings reduce the welfare-detrimental risk of political influence and decrease uncertainty for the firms, the desirability of a merger control system comprised of clear-cut, transparent and traceable rules and proceedings has long been stressed by scholars and practitioners (e.g. Smith (1957) and Elman (1965)). The benefits of legal certainty known to the literature are numerous: it increases the credibility of the authorities by encouraging them to base their decisions on accurate facts and sound economic reasoning, making their rulings more consistent and fair. It fosters accountability and reduces personal biases in the decision process. It allows the concerned firms to better understand the merger review process and predict its outcome with a certain reliability, thereby discouraging them from proposing clearly welfare-reducing mergers.¹³ Thus, a predictable merger control process reduces the high costs and reputation losses entailed by a lengthy antitrust procedure (Neven, Nuttall, and Seabright (1993)). Moreover, the transparency of legal procedures increases the potential for harmonization among multiple regulatory authorities. Transparency can, however, also entail costs (McAfee (2010)). By encouraging simple rules it might lead to repeated consistent errors, which could be avoided by using more flexible, case-by-case criteria allowed by a rule-of-reason approach. It increases the costs borne by antitrust authorities

¹³In a slightly different setting, Barros (2003) theoretically proves that an increase in the uncertainty of the antitrust policy's implementation leads to more anti-competitive agreements to be proposed by firms.

by imposing them to disclose the outcomes and the economic and legal reasoning motivating their decisions. Finally, it might also delay innovations in the adoption of more sophisticated and precise assessment techniques by creating path dependency with respect to past decisions.

Yet, a balance of these costs and benefits reveals undoubtedly that transparency, especially with concern to the process underlying antitrust evaluation, is desirable.¹⁴ The transparency of the proceedings of a competition authority can be classified on the basis of the comprehensiveness and public availability of documents containing the economic reasoning in merger cases, guidelines for their handling, and detailed statistics. In general, the level of transparency of European merger control has been found to be laudable in comparison with that of other competition authorities (e.g. Gelfand and Calsyn (2005)).¹⁵

The impact of ECMR 04 on the predictability of merger decisions, however, is *ex ante* ambiguous. On the one hand, the publication of merger guidelines and several institutional changes were clearly aimed at augmenting legal certainty. On the other hand, the more intensive use of specific theoretical and econometric tools, aimed at accurately pinning down the specificities of each

¹⁴Shapiro (2010), for instance, reports that the US Assistant Attorney General "explained [...] that a major goal of revising the [US merger] Guidelines [in 2010] was to provide greater transparency [...] and reduce the gaps between the Guidelines and actual agency practice—gaps in the sense of both omissions of important factors that help predict the competitive effects of mergers and statements that are either misleading or inaccurate."

¹⁵DG Competition itself is concerned with the transparency and predictability of its proceedings. For instance, a press release on January, 6, 2010 reports: "Detailed explanations concerning how European Commission antitrust procedures work in practice have just been published by the Commission's Directorate General for Competition (DG Competition) and the Hearing Officers on the Europa website in order to further enhance the transparency and the predictability of Commission antitrust proceedings. The explanations are outlined in three documents, namely Best Practices for antitrust proceedings, Best Practices for the submission of economic evidence (both in antitrust and merger proceedings) and Guidance on the role of the Hearing Officers in the context of antitrust proceedings. The documents will make it easier for companies under investigation to understand how the investigation will proceed, what they can expect from the Commission and what the Commission will expect from them." (see: <http://europa.eu/rapid/pressReleasesAction.do?reference=IP/10/2>)

single case, makes singular decisions more difficult to be anticipated, since the decision process is less anchored on simple, general rules (e.g. Kobayashi (1997)).¹⁶

As discussed by Voigt and Schmidt (2005), predictability can be defined as the "capacity to make predictions concerning the actions of others that have a high chance of turning out to be correct." Before proposing a concentration, the involved firms should be able to predict to a large extent the reaction of the competition authority on the basis of observable characteristics related to the merger. Therefore, one testable implication of legal certainty in merger control is the predictability of the EC's decisions.¹⁷

Thus let P_j be the actual decision taken by the agency on merger j , which is equal to 1 when the merger is remedied or blocked, which we will call *action*, and zero otherwise (*clear*). Let X_j be a set of observable characteristics related to the specific merger. These might be characteristics of the merging firms, the product and geographical markets where they operate, the nature of the merger they propose, as well as the merger policy history up to the point in time when merger j is proposed. We measure the predictability of the decision on the basis of goodness of fit measures of the following regression:¹⁸

$$P_j = \alpha_0 + \alpha_1 X_j + \varepsilon_j \quad (2)$$

¹⁶As noted by Christiansen (forthcoming): "[...] with the simultaneous introduction of unilateral effects analysis and the 'efficiency defense', it is possible not only for mergers 'below' the previously relevant market dominance threshold to be prohibited but also for mergers 'above' it to be approved. This boils down to a wider margin of discretion in decision-making, thus making the Commission's decisions permanently more difficult to predict."

¹⁷Similar analyses have been performed by Bergman, Jakobsson, and Razo (2005), and Bougette and Turolla (2006). Yet, the logic of their work is not motivated by the concept of predictability but rather by the aim of providing a test of whether the antitrust authorities give appropriate weights to the factors that they regard as important ex ante, such as market shares, concentration, and barriers to entry.

¹⁸We assume that the error terms ε_j are correlated over time, so we cluster them at the year level.

We would expect changes in legal certainty to be accompanied by changes in the predictive power of model (2), measured by standard statistics such as the *pseudo* – R^2 , the percentage of correct predictions, the sensitivity (i.e. the proportion of actual positives (here interventions) which are correctly identified), and the specificity (i.e. the proportion of actual negatives (here outright clearances) which are correctly identified).

Type I and Type II Errors

The first assessment of a particular decision is whether it conforms to the objectives of merger control and, hence, whether the Commission committed mistakes. According to our discussion in section , a benevolent agency intervenes in a merger if and only if consumer surplus (CS) would be reduced, hence the optimal decision rule for merger j is:

$$D_j = \begin{cases} 0 & \text{(clear) iff } CS_j \geq 0 \\ 1 & \text{(intervention) iff } CS_j < 0 \end{cases}$$

Let P_j again be the actual decision taken by the agency on merger j , which is equal to 1 if the merger is remedied or blocked, and zero otherwise. We say a type I error occurs if the agency intervenes in a merger that should have been cleared without commitments, i.e. $E1_j = 1$ if $P_j = 1$ and $D_j = 0$, else 0, and a type II error when the agency clears a merger that should have been blocked or remedied, i.e. $E2_j = 1$ if $P_j = 0$ and $D_j = 1$, else 0.¹⁹ In order to measure $E1_j$ and $E2_j$, we need to measure D_j , which requires an estimate of the impact of the merger on consumer surplus. Under our maintained assumptions, consumer surplus decreases after the merger if the profits of the rivals to the merging firms increase. Hence, the consumer welfare-maximizing merger control decision is:

¹⁹The notion of type I errors we use here corresponds therefore to the *weak* type I errors in Duso, Neven, and Röller (2007). Given that prohibitions were a very rare event in the entire sample and, especially, in the post-reform period, it would be impossible to perform any econometric analysis on the *strong* type I errors, i.e. pro-competitive mergers which were blocked. We will come back to this important point in section .

$$D_j = 1 \text{ if } \Pi_{C_j}^{A*} > 0$$

where $\Pi_{C_j}^{A*}$ represents the corrected merger announcement CAARs of the competitors (C) for merger j .²⁰ Once we have defined type I and type II errors, we analyze their determinants by running the following probit regressions:

$$E1_j = \alpha_0 + \alpha_1 X_j + \varepsilon_j \text{ if } D_j = 1 \quad (3)$$

$$E2_j = \beta_0 + \beta_1 X_j + \varepsilon_j \text{ if } D_j = 0 \quad (4)$$

Clearly, if the agency is benevolent, then both errors should be completely random and, hence, neither the determinants X_j nor the constants α_0 and β_0 should have significant explanatory power to predict them. However, in a political economy model of merger control (e.g. Neven and Röller (2005)), the antitrust agency maximizes an objective function containing not only the welfare of the economy but also the additional utility that it can obtain from third parties. These include the involved firms and other agents such as member states' governments, which provide contingent perks or, more generally, other kinds of private benefits. The determinants of errors X_j are thus merger-specific characteristics and institutional and political economy variables that may influence the decision of an antitrust agency (see Duso, Neven, and Röller (2007)).

In this political economy context, the gains generated by the merger for the merging firms and their rivals can be seen as the amount of resources that these firms are willing to pay to obtain their preferred outcome.²¹ One would expect type I errors to decrease with the gains of the merging firms, as they are interested in obtaining a clearance for a pro-competitive merger, and to

²⁰Notice that, in order to identify whether deals are perceived as anti-competitive, we only use the sign of the expected change in the stock price. Hence, the fact that the market may anticipate the outcome of the antitrust procedure or any other bias in the *size* of the CAARs due to other market expectations, do not introduce a bias in our identification.

²¹We define the gains for the merging firms and rivals as the CAARs multiplied by their market value, i.e. the firms' cumulative aggregate absolute value change due to the merger.

increase with the gains of the rivals, as they would like to block such mergers. The interests of both kinds of firms are, instead, aligned in the case of anti-competitive mergers. Hence, the probability of a type II error should increase with the gains of the merging firms as well as with those of the rivals, if they manage to lobby for their favorable decision.

Second, the size of the country from which the merging firms originate could also play a role in the outcome of a merger investigation, presumably because of the political pressure that can be exerted by large countries (e.g. Neven, Nuttall, and Seabright (1993) and Horn and Levinsohn (2001)). Third, as shown by Aktas, de Bodt, and Roll (2007), the European Commission might be protectionist and favor European versus US firms, hence the country of origin of the merging parties might be a determinant of the EC's mistakes. Fourth, the EC was often alleged to define relevant markets too narrowly, which might imply a higher frequency of errors (Neven, Nuttall, and Seabright (1993)). Fifth, procedural issues, such as the time available to undertake the merger analysis, may be important. In particular whether the case has been decided in phase 1 instead of being subject to a more substantial investigation (phase 2) might increase the likelihood of errors. Sixth, a full merger as compared to a partial merger or a joint venture might be seen as more problematic since the anti-competitive effects that it generates might be expected to be larger (e.g. Bresnahan and Salop (1986) and Gugler and Siebert (2007)), whereas a cross-border merger might be treated more leniently since the market power aspects might be less problematic (e.g. Neary (2007)). Seventh, the pattern of errors may vary across the sectors in which the mergers are taking place, as some industrial sectors are more concentrated and/or have more political influence than others. Finally, the EC may learn over time how to implement more precise and effective remedial actions (Duso, Gugler, and Yurtoglu (2011)) and, therefore, the proclivity of errors might be a function of the history of merger control decisions and of time.

To assess how the institutional changes introduced together with the new merger regulation affected the likelihood and determinants of the EC's mis-

takes, we run the basic regressions 3 and 4 separately on the pre- and post-reform sub-samples.

Rent-Reversion

The next step is to assess the ability of different policy tools to effectively reduce the market power effects of a merger and, at the same time, to maintain the benefits to consumers generated by increased efficiency. The logic behind the approach developed by Duso, Gugler, and Yurtoglu (2011) is that there should be a reversion of the (anti-competitive) rents measured around the merger announcement due to the decision, if the antitrust action is effective. This implies that decision CAARs should be systematically negatively related to announcement CAARs when a decision is effective. We therefore assess the effectiveness of an antitrust action by running the following regression separately for merging firms and rivals:

$$\Pi_{fj}^{D*} = \sum_d \alpha_{fd} + \sum_d \beta_{fd} \Pi_{fj}^{A*} + \gamma_f X_j + \epsilon_{fj} \quad (5)$$

where Π_{fj}^{D*} is the probability-corrected decision CAAR of merging firms ($f = M$) and competitors ($f = C$), respectively, for merger j , while Π_{fj}^{A*} is the probability-corrected announcement CAAR. We estimate different intercepts (α s) and slopes (β s) for the different decisions ($d=clearance, behavioral remedies, structural remedies, or prohibition$).²² In all specifications we control for time and industry effects (manufacturing and communications), as well as some merger-specific characteristics (full and conglomerate mergers).

Duso, Gugler, and Yurtoglu (2011) explain in depth the sizes and signs of the intercepts and slopes, which are expected if merger control is perfectly

²²We distinguish remedies in two broad categories: behavioral and structural. Behavioral remedies contain all obligations concerning the conduct of the merged entity vis-à-vis its competitors. These include access to key facilities, supply guarantees and the disentanglement of intertwined directorates. Structural remedies mostly concern the divestiture of a business branch or a production site. Past empirical research suggests the superiority of structural remedies over behavioral ones - e.g. the in-house studies of the EU Commission (European Commission (2005)) and the FTC (Baer (1999)).

effective and under our maintained assumptions. The key points are, however, summarized below.

Prohibitions. The most extreme action taken by the EC, i.e. to block the merger, dissipates all rents, i.e. both the market power and the efficiency rents. Therefore, the slope coefficient should be $\beta_{prohib} = -1$ for merging firms and rivals. Moreover, the regression line should pass through the origin: if the merger does not generate any rents, no rents can be reverted by the decision, thus $\alpha_{prohib} = 0$.

Clearances. If the merger is cleared without commitments, we do not expect decision effects that are systematically related to announcement returns, thus $\alpha_{clear} = 0$ and $\beta_{clear} = 0$ for merging firms and rivals. This does not need to be the case if the reaction around the decision date conveys good news to the market about the feasibility of future mergers. In this case, we expect positive constants and/or slopes for the rivals if the clearing of the merger signals a green light from the EC to mergers in that particular industry. This is even more likely if the EC makes type II errors and unconditionally clears anti-competitive mergers.

Remedies. The situation is more complex in the case of remedies. Only market power rents should be dissipated by the antitrust decision if it is effective. Hence, each remedial action should entail a negative decision effect for merging firms and rivals. Hence, for rivals, we expect a negative intercept as well as a negative slope. The former captures the shift due to the elimination of the market power rents, while the latter indicates that rent-reversion should be larger, the larger the size of the market power rents generated by the merger. For the merging firms, instead, since both market power and efficiency effects are positive, we only expect a negative slope, while we expect a zero intercept.

We run separate regressions for the pre- and post-reform periods to assess the impact of the new merger regulation.²³ The variables contained in the vector X_j are again exogenous controls, such as year and industry dummies, and other merger-specific characteristics.

²³Only two cases were blocked post-ECMR 04, so we had to drop prohibitions from the effectiveness regressions.

Deterrence

Up to this point we have focused on the ex ante (predictability) and *in-fieri* effects (decision errors and rent-reversion) of merger policy. However, as pointed out by Sørsgard (2009) an optimal merger policy also entails ex-post effects since it involves deterrence. In particular, he shows that there is an optimal level of enforcement where some actions, which in isolation would be welfare detrimental, might be optimally taken to achieve deterrence and thus increase overall welfare. Hence, the role of deterrence is especially important if competition authorities commit errors and if their remedies are not always and completely effective. If this was not the case and merger policy was perfectly effective, then firms would know ex ante that every anti-competitive merger would be blocked or effectively remedied by the antitrust authority and, therefore, they would not even attempt to propose such combinations. Moreover, in the absence of type I errors, firms would always propose a pro-competitive merger knowing that it would always be cleared and that over-deterrence would not be an issue. Hence, the existence of decision mistakes is a key ingredient in a deterrence model.

Very few studies have empirically analyzed deterrence in merger policy. Seldeslachts, Clougherty, and Barros (2009), by using a panel of antitrust jurisdictions over the period 1992-2003, find that prohibitions deter future merger activity, as measured by the number of merger notifications, while remedies do not. Hence, they exclusively focus on the frequency aspect but cannot say whether merger policy over-deterred pro-competitive mergers. The question of the composition of mergers which are deterred by the policy is analyzed in a sequent paper (Clougherty and Seldeslachts (2010)). They analyze the US merger policy in the period 1986-1999 and show that second-request investigations and, even more so, antitrust interventions yield significant deterrence effects for horizontal mergers, i.e. negatively affect the ratio between horizontal to total notified mergers. They conclude that a tougher merger policy makes firms move away from potentially problematic horizontal mergers toward vertical mergers that are more likely to be efficiency-increasing. Yet, such

identification is quite simplistic since many horizontal mergers might involve efficiency-enhancing synergies. Key to the analysis of deterrence in merger control is that a good policy should deter firms from proposing socially detrimental mergers but it should not over-deter and hence discourage firms from proposing efficiency-increasing combinations.

Our analysis takes an important step in the direction of analyzing this issue and focuses on measuring 'good' deterrence. This is enabled by the unique information contained in our dataset that allows a much cleaner definition of the competitive nature of each merger and a finer prediction on the quality of deterrence achieved by the policy. For each merger we use the indicator of its anti-competitive nature ($D_j = 1$ if $\Pi_{C_j}^{A*} \geq 0$) and test for the deterrence effects of merger policy by looking at how past decisions affects the probability of a particular merger to be anti-competitive. In particular, we look at how the complete merger policy history of the European Commission affected the anti-competitiveness of the mergers in our sample. We thus combine measures of DG Comp's merger policy from the population of over 3,800 mergers scrutinized in the sample period with our dataset to estimate a probit equation of the following type:

$$D_{jt} = \alpha_0 + \alpha_1 n_{jt-2} + \sum_d \alpha_{2d} \frac{d_{jt-2}}{n_{jt-2}} + \alpha_3 X_j + \epsilon_j \quad (6)$$

where D_{jt} is equal to 1 if merger j proposed in quarter t was anti-competitive. The variable n_{jt-2} is equal to the total number of notifications to the EC two-quarters before merger j was notified, and $\frac{d_{jt-2}}{n_{jt-2}}$ represents the ratio of the total number of mergers with outcome d (where $d = \text{remedies, blockings, or withdrawals}$) over the total number of notified mergers lagged by two-quarters. Again, we control for other merger specific determinants X_j as we do in the other regressions.

The lagged number of notifications controls for merger wave effects. While several studies show that merger waves can be driven by periods of over- and undervaluation of the stock market (e.g. Gugler, Mueller, and Weichselbaumer (2011), Rhodes-Kropf and Viswanathan (2004) and Harford (2005)), very few

studies have looked at how merger waves might impact the competitive effects of a merger (e.g. Gugler, Mueller, and Yurtoglu (2005) and Clougherty and Duso (2009)). One might argue that on the wave-crest the quality of the targets and the fit of the match between acquirer and target is worse. Hence, less efficiency-enhancing and potentially more anti-competitive mergers will be proposed.

More importantly, the kind of merger policy decisions and their effectiveness send signals to firms about the toughness of the authority. If merger policy deters anti-competitive mergers, one should expect negative coefficients for all kinds of actions. Yet, as shown by Seldeslachts, Clougherty, and Barros (2009), the kind of signal a particular decision sends to the firms and, hence, the kind of merger the firms propose, crucially depends on the expectations the firms have about the merger policy. It is quite clear that prohibitions have a deterrence effect, as they represent the toughest action an antitrust authority can take. Similarly, one could argue that when the merger parties withdraw or abort a notified merger, this might be interpreted as an 'almost-prohibition' (Bergman, Jakobsson, and Razo (2005)) and, therefore, this can be expected to have similar deterrence effects. The deterrence effects of remedies are not so clear cut and depend on whether they are effective and whether they come at the expense of clearances or prohibitions. First, ineffective remedies cannot strongly deter as they do not constitute a real threat for firms which want to propose anti-competitive combinations. Second, if the antitrust authority becomes tougher and imposes remedies on mergers which were expected to be outrightly cleared, then the firms will see this as a negative signal and, potentially, some anti-competitive mergers will not be proposed. If, instead, the authority applies remedies on mergers which were expected to be blocked, the firms may update their beliefs, deducing that merger control has become more lenient and, therefore, propose more anti-competitive mergers. We will use the difference between phase 1 and phase 2 remedies to shed some light on these issues.

We estimate regression (6) separately for the pre- and post-reform periods to identify the potential effect of the reform. Notice that, similarly to the

rent-reversion regressions, we are not able to identify the effect of prohibitions post-reform, since only two mergers were blocked after 2004.

Data

Data Sources

Our sample includes 326 merger cases scrutinized by the EC from the beginning of 1990 to the end of 2007. We collected information on all phase 2 mergers during the sample period, together with a randomly matched sample of phase 1 merger cases. The first half of the sample (157 mergers from 1990 to the end of 2002) was first developed by Duso, Neven, and Röller (2007), whereas the 169 cases for the 2003-2007 period were newly collected and evaluated for this paper.²⁴ By carefully reading the text of publicly available merger cases handled by DG Comp, we identified the merging parties, their rivals, relevant markets, decision types, the dates of the notification, phase 1 and possibly phase 2 decision, and some other merger-specific characteristics.²⁵ From these decisions, we identified a total of 3,026 involved parties, 735 of them are merging parties and 2,291 are competitors. Due to data limitations and the requirement that firms have to be listed, the final sample contains 1,771 of these firms (522 merging parties and 1,249 competitors). Since there are overlaps in the firms' roles as merging parties and competitors, i.e. the same firm appearing as a merging party and/or competitor in multiple cases, our sample includes 1,104 unique firms.

Using the EC's merger assessment to identify the rivals represents a particular strength of this sample. It has the big advantage of being a much more realistic description of the relevant markets than, say, using SIC codes, which would yield a sample of firms active in the same branch, but possibly not

²⁴While, for the first part of the sample, phase 1 cases were taken completely randomly, we over-sampled the phase 1 cases cleared with remedies in the 2003-2007 period. We did this because we were particularly interested in analyzing the effects of actions in phase 1.

²⁵All documents are publicly available at <http://ec.europa.eu/competition/mergers/cases/>.

competing in the specific product market concerned in a merger. This is particularly true for large, integrated corporations, which are active in numerous sectors (e.g. Eckbo and Wier (1985)).

Following Banerjee and Eckard (1998), the announcement date of a merger is defined as the date on which first rumors of that particular merger leaked to the market. This is usually before the official notification to the EC as well as the official merger announcement. We used the financial press and the Dow Jones Interactive database to identify the dates at which the first definitive indications of the combination between the merging parties became known.²⁶ The total return index, market value and branch index time series for the identified parties were downloaded from the Thomson Reuters Datastream database, providing daily data for the variables in question.

Summary Statistics

Table 1 summarizes the dummy variables partitioning our dataset for the periods before and after the merger policy reform in our sample and in the population.

In our sample, the percentage of cases that were cleared with remedies increases from 42% in the pre-reform period to 60% post-reform. If compared to the population data (7% pre-reform to 6% post-reform), remedied cases are clearly over-sampled. This is due to two facts. First, we over-sampled phase 2 cases. Second, for the period 2002-2007, we also over-sampled remedied cases in phase 1, which explains their increase from 14% to 40%, while in the population data they are constant at a rate of 4%. Our sample, however, mimics the population data for what concerns the use of remedies in phase 2. They decrease from 28% to 14% from the pre- to the post-reform period in

²⁶As a robustness check, we collected data on the merger's official announcement date from the SDC database (Thomson Reuters). Unfortunately, this database turned out to be incomplete and we were able to identify only 240 of our original mergers. Most of the official announcements are in an interval around five days before and two days after the first rumors. For some 20% of the cases the first rumor was 15 days or earlier before the official announcement.

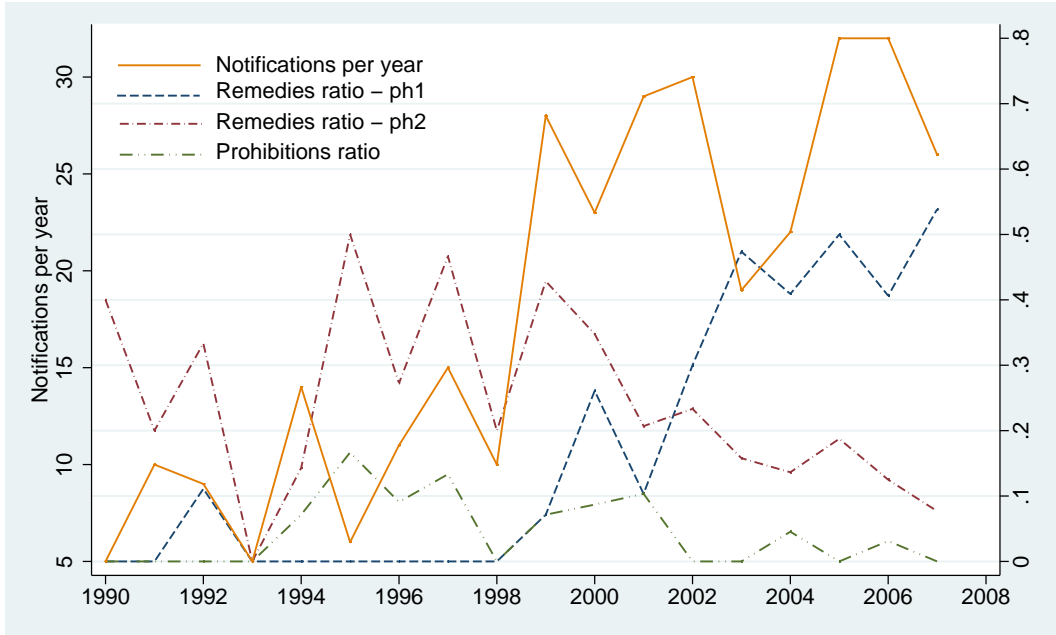
our sample and from 3% to 1% in the population. Prohibitions represent 6% and 2% of the cases pre- and post-reform in the sample, and 0.8% and 0.06% in the population. All other cases have been cleared without conditions and obligations. For the population data, we also have information on aborted or withdrawn cases. These represent 3% and 2% of the notified cases pre- and post-reform, respectively.

Table 1: Summary Statistics of Dummies

	Sample				Population			
	Pre-		Post-reform		Pre-		Post-reform	
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
Remedies	0.421	(0.49)	0.600	(0.49)	0.069	(0.05)	0.055	(0.04)
in Phase 1	0.144	(0.35)	0.464	(0.50)	0.041	(0.04)	0.044	(0.03)
in Phase 2	0.278	(0.45)	0.136	(0.34)	0.028	(0.03)	0.011	(0.01)
Cleared	0.523	(0.50)	0.382	(0.49)	0.931	(0.05)	0.945	(0.04)
Prohibited	0.056	(0.23)	0.018	(0.13)	0.008	(0.01)	0.001	(0.00)
Phase 2 cases	0.421	(0.49)	0.236	(0.43)	0.055	(0.23)	0.026	(0.16)
Aborted/Withdrawn					0.033	(0.03)	0.024	(0.01)
Remedies: Divestiture	0.287	(0.45)	0.455	(0.50)				
in Phase 1	0.097	(0.30)	0.336	(0.47)				
in Phase 2	0.190	(0.39)	0.118	(0.32)				
Remedies: Behavioral	0.259	(0.44)	0.245	(0.43)				
in Phase 1	0.065	(0.25)	0.182	(0.39)				
in Phase 2	0.194	(0.40)	0.064	(0.25)				
National markets	0.384	(0.49)	0.418	(0.50)				
EU-wide markets	0.407	(0.49)	0.373	(0.49)				
Worldwide markets	0.204	(0.40)	0.200	(0.40)				
Conglomerate merger	0.250	(0.43)	0.582	(0.50)				
Full merger	0.579	(0.49)	0.709	(0.46)				
US firms involved	0.315	(0.47)	0.191	(0.39)				
Observations	216		110		2402		1634	

Remedies represents the fraction of cases that were either cleared subject to conditions & obligations (blockings are reported under *Prohibited*). *Cleared* is the fraction of cases that were permitted without conditions & obligations, *Phase 2 cases* were subjected to an in-depth investigation. *Aborted/Withdrawn* are transactions that were notified to the EC, but withdrawn during the review procedure. *National*, *EU – wide* and *Worldwide* markets refer to the market definitions applied by the EC, *Conglomerate mergers* involve conglomerate, vertical, or foreclosure effects, *Full mergers* means that one of the merging firms was acquired in its entirety. *US firms involved* equals one if one of the merging firms has its headquarter in the US.

Figure 1: Evolution of cases and decisions in our sample

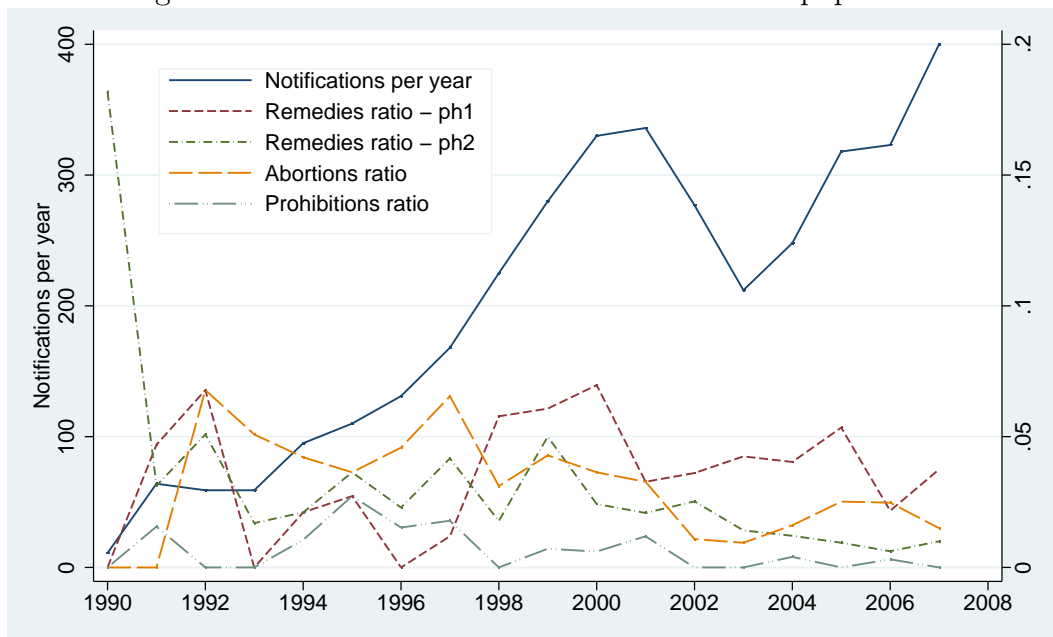


We report the yearly average of notified cases as well as the ratio of different decisions (remedies in phase 1 and phase 2, prohibitions) to the notified cases in our sample.

Figures 1 and 2 show the time evolution of notifications and actions in the sample and in the population respectively. In both the sample and the population, we observe an increasing trend in notifications with a single big drop around 2002. The proportion of remedies in phase 2 oscillates before 1999 to then take a downward trend, while the proportion of remedies in phase 1 increases substantially. The prohibitions ratio displays a downward trend, with only 2 prohibitions after the merger reform. Abortions or withdrawals stay at a rate of 4% to 6% until 2002, then slightly decrease to increase again in 2005 and 2006.

Finally, for the mergers in our sample, we have more precise information about the cases' specificities (bottom part of table 1). From the pre- to the post-reform period, we observe an increase in divestitures (from 29% to 45%), a small decrease in behavioral remedies (from 26% to 25%), a tendency to use a narrower market definition (more national and fewer EU-wide markets), more conglomerate and full mergers, and less involvement of US firms.

Figure 2: Evolution of cases and decisions in the population



We report yearly average for the number of notified cases as well as the ratio of different outcomes (remedies in phase 1 and phase 2, prohibitions, abortions) to the notifications in the population of all EU merger cases.

Structure of the Cumulative Abnormal Returns

We calculate the CAARs over the event windows according to the methods presented in Appendix A. We use the total return index from Datastream, which accounts for dividends and corrects for stock splits. Table 2 reports mean values for merging firms and rivals, in the pre- and post-reform periods respectively.

On average, the mergers in the sample are profitable for merging firms pre-reform and yield an increase in their stock value of around 1.6%, which is significant at the 5% confidence level. After the reform, mergers are still significantly profitable for merging firms yet the average CAAR drops to 1.1%. The impact of DG Comp's decisions on the valuation of merging firms is very similar pre- and post- reform and entails a negative, but not significant, drop in the firms' stock value by 0.3% and 0.5% respectively.

Table 2: CAARs of merging parties and rivals by period and event

		Pre-Reform			Post-Reform	
	N	Mean	S.E.	N	Mean	S.E.
Merging Firms						
Announcement	200	0.016**	(0.009)	96	0.011*	(0.008)
Decision	197	−0.003	(0.008)	96	−0.005	(0.005)
Rivals						
Announcement	208	0.008	(0.007)	105	0.011*	(0.007)
Decision	207	−0.003	(0.009)	105	−0.009	(0.007)

The symbols ***, **, and * represent significance at the 1%, 5%, and 10% levels respectively.

The competitors' merger announcement effects are positive (0.8%) but not significant prior to the reform, and they increase to 1.1% and become significant at the 10% significance level post-reform. Similar to the merging firms, rivals suffer an average negative reaction around the EC decision, which ranges between 0.3% pre-reform and 0.9% post-reform. In both cases, however, the average CAARs are not statistically different from zero.

Empirical Results

Probability of Intervention

The first piece of evidence on the effects of the merger policy reform relates to the predictability of antitrust intervention. The regression results of the probit model (2) are displayed in table 3. We report the marginal effects of different sets of observable factors that we expect to explain the intervention of the EC on a specific case. Before looking at the coefficient estimates, we first focus on the predictive power of the probit regressions pre- and post-reform. We then discuss in brief the main determinants of an action and how they changed over time.

The first observation is that the *pseudo*– R^2 pre-reform (0.51) is more than twice as large as post-reform (0.23). This implies that our model explains

Table 3: Probit Model: Probability of Intervention

	Pre-Reform		Post-Reform	
US firms involved	−0.425***	(0.098)	−0.424***	(0.077)
EU firms involved	−0.291***	(0.105)	0.110*	(0.065)
Big EU country	0.039	(0.107)	−0.361**	(0.176)
Conglomerate merger	0.455***	(0.139)	0.400	(0.283)
Full merger	0.310***	(0.114)	−0.209**	(0.088)
Crossborder merger	0.003	(0.110)	0.130	(0.115)
Log(MV) merging firms	0.043***	(0.010)	−0.012	(0.007)
Log(MV) rivals	0.034**	(0.014)	0.033*	(0.017)
Rival announcement CAAR	−0.525	(0.390)	0.720	(0.714)
Phase 2 Case	0.780***	(0.069)	0.049**	(0.023)
Worldwide markets	−0.186	(0.164)	−0.063	(0.109)
National markets	0.435***	(0.088)	0.581**	(0.239)
Number of regions involved	0.053	(0.066)	−0.110***	(0.042)
Number of markets involved	0.103	(0.077)	0.032	(0.065)
Mining	0.287	(0.262)	−0.371	(0.255)
Manufacturing	0.024	(0.169)	−0.045	(0.248)
Transport	0.364***	(0.111)	−0.240	(0.278)
Communication	0.134	(0.135)	−0.473***	(0.157)
Finance	−0.339**	(0.150)		
Service	−0.208	(0.211)	−0.398**	(0.175)
Lagged notifications	0.001	(0.003)	−0.014***	(0.005)
Lagged actions ratio	1.601**	(0.693)	5.453***	(0.990)
Lagged abortions ratio	0.153	(1.838)	−12.345***	(1.104)
Time trend	0.002	(0.007)	0.042**	(0.018)
Observations	203		105	
Pseudo R^2	0.51		0.23	
CorrClassified	87%		71%	
Sensitivity	86%		83%	
Specificity	87%		54%	

The dependent variable is action, defined as $P_j = 1$ when the merger is remedied or blocked, and zero otherwise. Marginal effects are reported. dF/dx is for discrete change of a dummy variable from 0 to 1. Standard errors in parentheses are robust and allow for correlation among observations from the same year. The symbols ***, **, and * represent significance at the 1%, 5%, and 10% levels respectively. The *EU firms* dummy indicates that one of the merging parties is headquartered in the EU. *Big EU* countries are Germany, France, Italy, Spain and the UK, *Crossborder mergers* involve at least two nations, *Log(MV)* refers to the logged market values of merging parties and rivals. For the *Rival announcement CAAR* see . The number of regions and markets variables count the (geographic) regions and (product) markets concerned by a merger. *Mining*, *Manufacturing*, *Transport*, *Communication*, *Finance* and *Service* are industry dummies. For the lagged population variables, see . The time trend is measured in quarters. The remaining dummies are defined at the bottom of table 1.

the behavior of the EU Commission pre-reform much better than after the reform. It appears that a merger control approach based more on a case-by-case analysis and relying less on clear-cut rules reduces the predictive power of the conventional 'external', observable determinants.²⁷

Looking at the other measures of goodness of fit, our model is able to correctly classify 87% of the observations pre-reform, whereas post-reform we correctly classify 71% of the observations. We can also identify where this difference comes from. There is very little difference pre-reform (86%) and post-reform (83%) in the ability of our model to correctly classify interventions (remedies and blockings) by the EU Commission measured by the sensitivity statistic. Post reform, however, the model is much less able to correctly classify the outright clearances, i.e. specificity. This happens in only 54% of the cases post-reform versus 87% of the cases pre-reform. Therefore, it seems that the reform reduced the predictability of an absence of an action by the EC based on measurable characteristics, however the reform did not reduce legal certainty in case of an intervention. As we shall see later, this is an indication of the generally more active stance of the EC.

We then look at the observable factors, which are significant predictors of the merger control outcome and identify some differences. A first set of variables that we consider is related to the 'political economy environment'. The country of origin of the firms involved in a merger case is an important driver of the EC's decision. In both periods, the probability of eliciting an action is over 42% lower if a US firm is involved. Also, there is a shift toward a significant reduction in the likelihood of an action if a big EU country was involved in the case post-reform. This confirms the argument put forward by Horn and Levinsohn (2001), who point to the effectiveness of the political pressure by large countries.²⁸ However, the likelihood of interventions increases post-reform if EU firms are involved, thus we cannot confirm the findings of Aktas, de Bodt, and Roll (2007) concerning protectionism.

²⁷Of course, we cannot rule out that any policy shift would reduce predictability for a certain time period, until the firms learn the rules and get acquainted to them.

²⁸The big EU countries are: France, Germany, Italy, Spain, and the UK.

Merger specific characteristics play a more significant role pre-reform, with a significant increase in the probability of eliciting an action for full mergers, and mergers between large firms and with large competitors. This corresponds to the intuition that full mergers are potentially more harmful to competition than mere share acquisitions or joint ventures. It also suggests that the EC was more suspicious of concentrations among large firms pre-reform, which can be seen as in accordance with the doctrine dictated by the dominance test. Post-reform, instead, full mergers are 21% significantly less likely to be remedied or blocked. While the size of the merging firms does not play a role anymore, the size of the competitors still significantly increases the likelihood of an action.

The anti-competitive nature of the merger should be the main driver of the antitrust decision. We use two different proxies for this unobserved factor. Our first indicator is the size of the announcement CAARs of the rivals, as the signals upon which the market bases its evaluation of a merger might also be available to DG Comp during its assessment of the combination. Even though the CAAR-coefficient assumes the expected positive sign after the reform, no statistically significant relationship emerges. Instead, the fact that the merger went to phase 2, which is our second indicator of the competitive issues raised by the merger, is a very significant predictor. Yet, sizeable effects are only seen pre-reform (an increase of 78% in the likelihood of an action), while post-reform the increase is by only 5%. This corroborates the fact that in more recent years the EC increasingly started to use remedies also in the early phase 1 investigation phase. The market definition is an important determinant of an intervention. If the relevant market is defined nationally, the likelihood of eliciting an action increases by over 43% pre-reform and 58% post-reform: smaller product markets are more problematic.²⁹ Consistently, post-reform, if the involved firms (merging and rivals) originate from more geographic world regions, the probability of intervention is significantly re-

²⁹It could, however, also be that DG Comp sometimes defines the relevant anti-trust markets too narrowly and makes mistakes, see section 4.2. and (Neven, Nuttall, and Seabright (1993)).

duced. Industry effects are significant pre-reform (transport and finance) and post-reform (communication).

Finally, we look at whether past decisions of the EC affect current decisions to capture possible learning or path-dependency effects. If the EC was very active in the last-but-one quarter, the probability of an action increases significantly. This points to some kind of path-dependency in the EC's decisions. The limited resources' argument seems to have bite post-reform, since the likelihood of an intervention decreases significantly with an increase of notifications in the last quarter. Post-reform, if there are more abortions in the previous last-but-one quarter, the EC steps in less frequently. This might be an indication that abortions or withdrawals have a deterrent effect post-reform, an issue to which we return later. We also observe a positive and significant time trend toward more interventions by the EC in the post-reform period.

As we explain in the appendix , we use the predicted values from these probit regressions to estimate the probability of an action $\hat{\rho}$, which is used to build our measure of corrected CAARs that will be used in the following steps of our analysis.

Type I and Type II Errors

The next piece of evidence on the effects of the merger control reform is the analysis of the EU Commission's possible mistakes in the enforcement of the merger regulation. We first look at simple frequencies, which are reported in table 4.

Table 4: Type I/II errors by period

	Pre-Reform			Post-Reform		
	Mean	S.D.	Count	Mean	S.D.	Count
Type I error	0.221	(0.416)	46	0.324	(0.470)	34
Type II error	0.269	(0.445)	56	0.181	(0.387)	19

Frequency of type I errors (action in a pro-competitive merger) and type II errors (unconditional clearance of an anti-competitive merger) in the sample.

Post-reform, we observe on average more type I errors (32% compared to 22%) and fewer type II errors (18% compared to 27%) than in the pre-reform period. This hints to the fact that the EC has become, on average, more active since the introduction of the new merger regulation: it intervenes more often in pro-competitive mergers and there are fewer anti-competitive cases without intervention. While we are fully aware that a welfare analysis of this policy shift is not possible in our context, we want to stress that we measure *weak* type I errors. Arguably, the cost generated by the application of a remedy on a merger which is pro-competitive on average is possibly smaller than the cost of letting anti-competitive mergers through completely unchallenged.

To analyze the determinants of type I errors (i.e. the EC intervenes in pro-competitive mergers), we use the variables discussed in the section on the theory of decision errors.³⁰ The results of regression (3) are reported in table 5.

First, we look at measures of governmental pressure. If one of the merging parties is a US-based firm, the likelihood of wrongly eliciting an action in pro-competitive mergers is, *ceteris paribus*, 76% lower in the pre-reform period and 57% lower after the reform. This reflects the results discussed above when we presented the determinants of an action. Apparently, thus, the EC is particularly cautious to avoid making type I errors when US firms are involved and this pattern does not change after the reform. We do not find any evidence of lobbying by firms, as expressed by the gains they obtain through the mergers, both before and after the reform.

Pro-competitive, full mergers are more likely to be wrongly remedied before the reform but less so after the reform. Before the reform, a type I error is significantly more likely when the merging firms and the competitors are large, while after the reform the size of competitors is not significant anymore. This might indicate that market shares matter less while economic reasoning matters more in post-reform EC decisions. Post-reform, cross-border mergers are more likely to be wrongly remedied, however. The market definition is

³⁰Notice that, for the post-reform period, we were forced to drop some variables because of collinearity problems due to the small sample size.

Table 5: Probit Model: Probability of Type I errors

	Pre-Reform		Post-Reform	
US firms involved	−0.761***	(0.142)	−0.565***	(0.188)
EU firms involved	−0.822***	(0.148)	0.804***	(0.147)
Big EU country	0.384**	(0.152)	−0.934***	(0.093)
Profit change - merging firms	−0.000	(0.000)	0.003	(0.002)
Profit change - rivals	0.000	(0.000)	−0.000	(0.000)
Conglomerate merger	0.114	(0.409)		
Full merger	0.573***	(0.190)	−0.720***	(0.222)
Crossborder merger	0.112	(0.227)	0.805***	(0.172)
Log(MV) merging firms	0.065***	(0.022)	0.172***	(0.052)
Log(MV) rivals	0.072**	(0.028)	−0.074	(0.046)
Worldwide markets	−0.646***	(0.111)	−0.883***	(0.071)
EU-wide markets	−0.722***	(0.125)	−0.863***	(0.132)
Number of regions involved	0.102	(0.138)	−0.323***	(0.073)
Number of markets involved	−0.006	(0.239)	0.366	(0.256)
Phase 2 case	0.971***	(0.044)	0.423*	(0.245)
Manufacturing	−0.635*	(0.350)	−0.619**	(0.265)
Communication	−0.460***	(0.123)	−0.786***	(0.170)
Service	−0.467***	(0.105)	−0.414*	(0.245)
Electricity	−0.431***	(0.108)		
Finance	−0.563***	(0.097)		
Transport			−0.223	(0.415)
Lagged notifications	−0.003	(0.009)	−0.039***	(0.014)
Lagged actions ratio	5.088	(4.144)	−0.880	(7.028)
Lagged abortions ratio	0.527	(2.714)	−39.688	(39.004)
Time trend	0.012	(0.018)	0.096	(0.071)
Observations	88		49	
Pseudo R^2	0.70		0.46	
CorrClassified	93%		82%	
Sensitivity	93%		93%	
Specificity	93%		67%	

The dependent variable is type I error defined as $E1_j = 1$ if $P_j = 1$ and $D_j = 0$, and zero otherwise (action in a pro-competitive merger). Marginal effects are reported. dF/dx is for discrete change of dummy variable from 0 to 1. Standard errors in parentheses are robust and allow for correlation among observations from the same year. The symbols ***, **, and * represent significance at the 1%, 5%, and 10% levels respectively.

again very important. The EC tends to commit significantly more type I errors if the market is narrowly defined (nationally), and the likelihood of a mistake significantly decreases if the market is defined to be EU-wide or world-wide. This pattern is reinforced post-reform. Again, this is evidence that DG Comp sometimes defines the relevant anti-trust markets too narrowly (e.g. Neven, Nuttall, and Seabright (1993)). Additionally, post-reform, the fact that the merger affects several geographic regions reduces type I errors. Both before and after the introduction of the new merger regulation, we find significant industry effects. Only after the reform, the EC's past decisions appear to affect the proclivity of type I errors. The likelihood decreases if the number of notifications increases: A large work-load might prevent the EC from being too pro-active in general and, in particular, against pro-competitive cases. The model's predictive power is very high both in terms of *pseudo*- R^2 and in terms of correct predications. Low values for the specificity statistic mostly explain why post-reform the percentage of correctly classified cases decreases from 93% to 82%.

We then move to the estimation of equation (4): the determinants of type II errors (i.e. the EC unconditionally clears anti-competitive mergers). The marginal effects of the probit estimations are reported in table 6.

We estimate significantly more type II errors in mergers involving US firms and merging parties coming from the big EU countries, but only post-merger reform. This hints at some form of political pressure by national governments. Moreover, we observe that the larger the gains for the merging firms, the lower the likelihood of eliciting an action in an anti-competitive merger, which is consistent with successful lobbying by merging parties. On the contrary, the coefficient for the rivals is negative and significant in the post-reform period. This goes against the idea that the rivals were also successful in lobbying the EC to let an anti-competitive merger be cleared. The negative sign might be interpreted as revealing that the EC is increasingly careful not to commit type II errors when the merger becomes more anti-competitive, i.e. the larger the profit gains for the rivals. This might also indicate that the EC takes stock market reactions increasingly into account in its decisions (see also Monti

Table 6: Probit Model: Probability of Type II errors

	Pre-Reform		Post-Reform	
US firms involved	0.149	(0.093)	0.899***	(0.090)
EU firms involved	-0.162	(0.113)	-0.282**	(0.135)
Big EU country	0.154*	(0.087)	0.711***	(0.216)
Profit change - merging firms	0.000***	(0.000)	0.005**	(0.002)
Profit change - rivals	-0.000	(0.000)	-0.000***	(0.000)
Conglomerate merger	-0.236***	(0.089)	-1.000***	(0.001)
Full merger	-0.306***	(0.104)	0.389*	(0.202)
Crossborder merger	0.062	(0.112)	0.246***	(0.063)
Log(MV) merging firms	-0.029*	(0.015)	0.248**	(0.098)
Log(MV) rivals	-0.025*	(0.014)	0.031	(0.031)
EU-wide markets	-0.223*	(0.135)	-0.271	(0.234)
National markets	-0.250*	(0.133)	-1.000***	(0.001)
Number of regions involved	-0.007	(0.049)	0.119**	(0.052)
Number of markets involved	-0.036	(0.082)	0.276*	(0.154)
Phase 2 case	-0.469***	(0.141)	0.921***	(0.097)
Manufacturing	0.212	(0.170)	-0.122	(0.315)
Communication	0.009	(0.187)		
Service	0.426	(0.599)	0.847***	(0.099)
Electricity	-0.050	(0.126)		
Trade	0.094	(0.213)		
Transport	-0.105*	(0.056)	0.534	(0.444)
Lagged notifications	-0.002	(0.001)	-0.006	(0.006)
Lagged actions ratio	-0.153	(0.514)		
Lagged abortions ratio	0.073	(1.122)		
Time trend	0.008***	(0.003)		
Observations	100		42	
Pseudo R^2	0.55		0.54	
CorrClassified	87%		90%	
Sensitivity	91%		88%	
Specificity	83%		92%	

The dependent variable is type II error defined as $E2_j = 1$ if $P_j = 0$ and $D_j = 1$, and zero otherwise (unconditional clearance of an anti-competitive merger). Marginal effects are reported. dF/dx is for discrete change of dummy variable from 0 to 1. Standard errors in parentheses are robust and allow for correlation among observations from the same year. The symbols ***, **, and * represent significance at the 1%, 5%, and 10% levels respectively.

(2008)). Post-reform, the EC seems to be too lenient toward full mergers, cross-border mergers, mergers involving large parties, and mergers concerning many world regions and product markets. The likelihood of type II errors increase in phase 2 post-reform, while pre-reform it decreases if the merger entered the in-depth investigation phase. This indicates that the EC became too hesitant to block or remedy mergers post-reform in phase 2. Again the predictions of the model are quite accurate with a *pseudo* – R^2 of over 50% and the percentage of correct predictions are close to 90% in both periods. Post-reform, specificity increased (from 83% to 92%) indicating that correct decisions (remedy or blocking) in anti-competitive mergers became increasingly more accurately predictable.

To conclude, our analysis so far shows that DG Comp became more proactive and therefore the composition of the mistakes changed toward more (weak) type I and fewer type II errors, particularly in phase 2. Post-reform, we find some evidence of successful political pressure by large EU countries and the US and lobbying by merging parties to get potentially anti-competitive mergers cleared. Post reform, the Commission became better at identifying anti-competitive mergers arguably due to the application of the more economics based approach.

Rent-Reversion Estimations

We now turn to the assessment of the effectiveness of different merger policy tools to reduce the anti-competitive rents generated by the merger. We estimate equation (5) for the merging parties and their rivals separately. The dependent variable, the probability-corrected decision CAAR, is regressed on different constants for the different decisions (*clearance*, *behavioral* and *structural remedies*, and *prohibition*), and on the interaction terms of decision type and probability-corrected announcement CAARs. These coefficients measure the rent-reversion achieved by the respective decisions of DG Comp. The regression results reported in table 7 for the pre-reform period are very

close to those obtained by Duso, Gugler, and Yurtoglu (2011) for the years 1990-2002.

We estimate a negative and significant slope of -0.92 for outright blockings for merging firms, which is also negative but not significant for rivals (-0.53). Both coefficients are not statistically significantly different from minus one. Hence, prohibitions seem to fully reverse the rents measured by the stock market around the announcement of the merger, and can be interpreted as being an effective merger policy tool.

Clearances have a positive and significant effect on merging firms as measured by the intercept, yet, only in the pre-reform period. This suggests that outright clearances send a positive, unexpected signal to the market. For rivals, the positive and significant slope estimates, both pre- and post-reform, imply that after an outright clearance their profitability increase with the size of their announcement rents, which we interpret as a measure of anti-competitiveness. Hence, our interpretation is that these findings are consistent with the market evaluating the cost of a type II error: the more anti-competitive the deal is (i.e. the larger the rivals' rents at announcement), the more rivals profit from an outright clearance. The coefficient estimates for remedies are also not in line with the predictions for an effective merger control. In particular, the predicted negative shift for rivals is not observed pre-reform, since the estimated intercepts are not significantly different from zero. For merging firms, we find divestitures to even produce a positive and significant shift.

Table 7: Effectiveness Regressions

	Pre-reform			Post-reform		
	Merging Parties	Rivals		Merging Parties	Rivals	
Clearance	0.244* (0.133)	0.010 (0.070)		-0.066 (0.096)	-0.071 (0.061)	
Behavioral	0.107 (0.155)	0.038 (0.074)		-0.015 (0.069)	-0.102* (0.041)	
Divestiture	0.330* (0.159)	0.035 (0.098)		-0.006 (0.055)	-0.093 (0.071)	
Prohibitions	0.309 (0.177)	-0.228 (0.160)				
Π_{ij}^{A**} Clearance	0.031 (0.141)	0.331** (0.148)		-0.069 (0.069)	0.611* (0.250)	
Π_{ij}^{A**} Behavioral	0.075 (0.247)	-0.069 (0.333)		-0.126 (0.095)	0.030 (0.056)	
Π_{ij}^{A**} Divestiture	-0.496 (0.309)	-0.141 (0.372)		0.234 (0.227)	-0.016 (0.100)	
Π_{ij}^{A**} Prohibition	-0.923** (0.316)	-0.532 (0.408)				
Observations	143	135		89	88	
R^2	0.26	0.20		0.35	0.21	

The dependent variable is the decision corrected CAAR in merger j (Π_{fj}^{D*}) for the merging firms ($i = M$) and competitors ($i = C$) respectively. Standard errors in parentheses are robust and allow for correlation among observations from the same year. We control for industry effects (manufacturing, mining electricity, trade, transport, communications), merger-specific effects (full and conglomerate mergers) and a time trend. The symbols ***, **, and * represent significance at the 1%, 5%, and 10% levels respectively.

To sum up, the main results pre-reform are: 1) prohibitions achieve full rent-reversion; 2) remedies do not achieve full rent-reversion on average; 3) outright clearances increase rents for merging firms and rivals, and the larger the merger effect at announcement the more they do so. Our interpretation of these findings in terms of merger policy effectiveness is: 1) prohibitions are an effective merger control tool; 2) remedies are not perfectly effective; and 3) some outright clearances might indeed be type II errors of the EC.

Looking at the post-reform sample, we only get two significant coefficients, the intercept for behavioral remedies and the clearance slope for rivals. Notice, however, that we cannot estimate the degree of rent-reversion achieved by prohibitions, since only two mergers were blocked post-reform. The negative intercept estimates in the case of remedies for rivals are indicative that remedies achieve, on average, some degree of rent-reversion post-reform. Yet, this reversion is not connected to the size of the announcement gains, since all slope estimates in the case of remedies are not significantly different from zero. The positive clearance slope for rivals again indicates that the market prices some of the Commission's type II errors.

The comparison of the scenarios before and after the reform suggest that, indeed, post-reform remedies have on average become more effective in reverting rents. However, we do not observe full rent-reversion, and outright clearances remain good news for rivals, which we interpret as being their benefit from type II errors. Given our result that prohibitions restore the pre-merger situation in the pre-reform sample and, hence, our interpretation that such policy instruments are effective, the reluctance of DG Comp to block mergers which was reinforced post-reform does not seem to be well-placed.

Deterrence Estimations

The last piece of evidence we propose relates to the deterrence properties of EU merger control. As mentioned in section , the deterrence properties of merger control are particularly relevant if type I and type II errors occur or remedies imposed by the antitrust authority are not always effective in reverting the anti-

competitive rents generated by the merger. This is exactly the situation that emerges from our results so far. To estimate the degree of 'good' deterrence achieved by the policy, we estimate model (6) and assess the likelihood that a newly notified merger is anti-competitive as a function of the history of past merger control decisions. The marginal effects of the probit estimations are reported in table 8.

Table 8: Deterrence Regressions

	Pre-reform		Post-reform	
Lagged notifications	−0.002	(0.004)	0.001	(0.002)
Lagged remedies ratio - ph1	−1.062*	(0.587)	−2.721	(3.143)
Lagged remedies ratio - ph2	2.909**	(1.259)	−0.554	(6.989)
Lagged abortions ratio - ph1	0.581	(1.401)	−9.673**	(4.464)
Lagged abortions ratio - ph2	6.756**	(3.350)	−14.072**	(7.009)
Lagged prohibitions ratio	−7.099*	(4.287)		
Observations	200		105	
Pseudo R^2	0.05		0.10	
CorrClassified	63%		63%	
Sensitivity	68%		61%	
Specificity	58%		64%	

The dependent variable is $D_j = 1$ if $\Pi_{Cj}^{A*} \geq 0$, zero otherwise. Standard errors in parentheses are robust and allow for correlation among observations from the same year. Marginal effects are reported. We control for industry effects (manufacturing, mining, services), merger-specific effects (full, cross-border and conglomerate mergers, relevant markets), firm-specific effects (country of origin, size) and a time trend. The symbols ***, **, and * represent significance at the 1%, 5%, and 10% levels respectively.

We estimate negative and significant coefficients for the intensity of remedies in phase 1 and for prohibitions in the pre-reform period. When the EC increases the use of these kinds of policy tools in the three to six months previous to a newly notified merger, its likelihood of being anti-competitive is significantly lower: these actions deter anti-competitive mergers. Prohibitions deter because they are the toughest policy tools. Remedies in phase 1 deter because they often come at the expense of expected clearances (e.g. Seldeslachts, Clougherty, and Barros (2009)) and because they are more clear cut and easy to implement and, hence, more effective than phase 2 remedies (e.g. Duso, Gu-

gler, and Yurtoglu (2011) and European Commission (2005)). Remedies and abortions in phase 2, on the contrary, do not deter anti-competitive mergers. They even increase the probability of anti-competitive mergers, possibly because they come at the expenses of a tougher action (prohibition) and therefore signal a soft antitrust stance by the EC.

Once again, we cannot test for the effects of prohibitions post-reform, as only two mergers were blocked after 2004. However, post reform, the ratios of withdrawn or aborted mergers in phase 1 and phase 2 have a negative and significant effect on the likelihood of a newly notified merger being anti-competitive. Moreover, both the remedy ratios in phase 1 and in phase 2 have a negative, though not significant, effect on the probability of a merger to be anti-competitive. After the reform, there was a clear policy shift toward a different use of merger tools. Prohibitions became a very rare event, and withdrawals or abortions appear to at least partially take over their deterrent role. One possible interpretation of these findings is that firms were pushed by the EC to withdraw particularly problematic mergers by setting the anti-competitive concerns at such a high level that any kind of remedy would have become too costly. Hence, these withdrawals/abortions might have been effective prohibitions.³¹

Robustness Checks

Purely Horizontal Mergers

As discussed in section , the correspondence between the change in consumer surplus and competitors profits does not necessarily hold for non-horizontal mergers. In all regressions we used a dummy to control for this issue, which is set equal to 1 for all those cases in which the Commission mentioned conglomerate, vertical, or foreclosure effects as one of its leading arguments in support of the final decision. In this section we discuss the results that we obtain by

³¹As noticed by Papanikolaou and Rosenthal (2011) "if the parties and the Commission are unable to agree on remedies, a fairly common result is the withdrawal of the notification to avoid the publication of a negative decision."

dropping all these 118 cases from our sample, which leaves us with 162 mergers pre-reform and 46 post-reform.³²

The results for the merger policy's predictability are only minimally affected in terms of sign and significance.³³ We observe some small changes in the size of the estimated marginal effect. Post-reform, the fit of the model is slightly increased even though some variables must be dropped because of collinearity due to the extremely small sample size (46 observations). The percentage of type I errors is higher than in the full sample (22% pre-reform and 43% post-reform) and a similar pattern is observed for the type II errors (33% pre-reform and 13.6% post-reform). Hence, the tendencies of a reduction of type II errors at the expense of an increased number of type I errors that we observed in the full sample are reinforced when considering purely horizontal mergers. The determinants of both types of errors are very comparable in terms of sign, significance, and size of the marginal effect pre-reform. While the market definition variables becomes not significant in the type II regression, in both type I and type II regressions the effect for the US becomes even larger and more significant. Due to the limited sample size, it is unfortunately not possible to estimate the determinants of the errors post-reform.³⁴

Also the rent-reversion regressions reflect the main findings observed in the full sample. The full rent-reversion achieved by the blocking decisions pre-reform is, as expected, even reinforced in the sub-sample of horizontal mergers – the coefficients are now -0.66 for the merging parties and -1.32 for the rivals which, in both cases, are not significantly different from -1. The

³²As an alternative definition for horizontal mergers, we also use a dummy that takes on the value of 1 if both merging parties share their primary SIC-classification codes. The two definitions overlap in 60% of the cases. According to this second definition, we drop 118 observations (54 pre-reform and 64 post-reform).

³³Due to lack of space, we do not report all tables. The extensive results can, however, be obtained from the authors upon request.

³⁴Using the second definition for horizontal mergers, the results pre-reform are very similar to those obtained for the full sample. The results for the post-reform regressions are in line with the main specifications, yet several coefficients are less precisely estimated and thus less significant due to the small sample size.

sign and size of the other coefficients are comparable to those from the main specification, hence the results are not qualitatively affected. Also in this case, the same qualitative results are attained using the second definition of horizontal mergers.

Concerning the deterrence regressions, the pre-reform results from the full sample are almost perfectly matched for purely horizontal mergers. Post-reform, some of the coefficients estimates become not significant due to the small sample size, even though the sign and size of the coefficients are not affected. Overall, we can therefore conclude that focusing on purely horizontal mergers does not alter our qualitative results, even though the smaller sample size in some cases affects the precision of the estimation and hence the significance level, especially post-reform.

The Timing of the Reform

To identify the effect of the reform, we choose the official date in which it legally came into force as a marking point for the pre- and post-reform periods. This choice of timing has a clear justification, since the EC could not have used the legal framework provided by ECMR 04 before this date. However, there might be reason to think that the right timing to assess the change in policy could have been before or after this date. On the one hand, it could have been before, because some of the reform's elements were implemented during the months antecedent the legal introduction of the new merger regulation and could have affected the Commission's policy enforcement.³⁵ On the other hand, the right timing to start the reform's assessment could also have been after May 2004, since it might have taken time before some of the innovations brought by the reform had a clear policy impact. Hence, we propose two robustness checks for this issue. First, we date the starting of the post-reform period back to the beginning of 2003. Second, we eliminate the entire year 2004 from the sample.

³⁵Lyons (2004), for instance, notices that several changes in merger control were being implemented around 2003, such as the introduction of devil's advocate panels, the proposal of a clarification of the dominance test, the appointment of the first chief economist, the publishing of the draft merger guidelines and the extension for timetable for remedies.

In both cases, the results on the predictability of the policy pre-reform do not change materially. The frequency of type I and type II errors is also not strongly affected by the selection of a different date or sample for the introduction of the reform. The determinants regressions for type I errors show similar coefficients estimates to the main specification, some of which are however less significant (EU firms's involvement, regions and markets involved, industry dummies). A similar pattern results from the type II regressions in both robustness checks. However, the post-reform results are much more consistent with our main specification when we exclude the entire year 2004. Again, this suggests that the change in the Commission's behavior around the time of the legal introduction of the reform in 2004 is particularly important in the analysis of the determinants of the Commission's errors.

The rent-reversion regressions are not strongly affected by the change in the timing of the introduction of the ECMR 04 if not for a drop in significance in the post-reform regressions in both robustness checks, where no coefficient estimates are significant. Finally, choosing the beginning of 2003 as the introduction year does not change any of the signs of the coefficients estimates both pre- and post-reform in the deterrence regression. Yet, it leads to a drop in significance, especially for prohibitions pre-reform and withdrawals post-reform. However, when we exclude the entire year 2004, all the findings obtained in our main regressions are perfectly mirrored in the robustness checks and significance is restored.

All in all then, it seems that our qualitative results also hold if we adopt another date for the formal introduction of the merger policy reform. However, results are much more significant, clear cut and in line with our main specification when we exclude the year 2004. This suggests that the change in policy around the date of the legal introduction of ECMR 04 was substantial and supports our choice as the most precise way to identify the effects of the reform.

Conclusion

In our attempt to assess the economic impact of the change in legislation due to the 2004 merger policy reform in Europe, we brought forward four pieces of evidence: (1) estimations of the determinants of intervention, (2) estimations of the frequency and determinants of type I and type II errors, (3) estimations of rent-reversion by merger decisions, and (4) estimations of the deterrence effect of merger decisions. These elements are thought to provide a comprehensive evaluation of the entire process of merger control: from an *ex ante* perspective on the predictability of the policy, to an *in-fieri* analysis of the effects of particular merger tools, to finish with the *ex-post* view of the effects of current policy enforcement on future firms' behavior. The identification of the reform's effects is achieved by comparing the performance of merger control along these four dimensions in the pre-reform and post-reform periods.

Our main findings can be summarized as follows. First, we find mixed evidence concerning the predictability of DG Comp's decisions. On the one hand, the reform did not decrease legal certainty in problematic mergers (i.e. anti-competitive mergers). On the other hand, it has become significantly more difficult to predict if the EC will unconditionally clear a merger. This might be partially due to the fact that it might take some time for firms to get acquainted with a new policy. Second, there is a tendency toward more interventions, especially a larger use of remedies, post-reform, resulting in fewer type II errors at the expense of more (weak) type I errors. The new approach of the EC, which is more clearly anchored on economic principles, appears to allow a better identification of the problematic cases. According to our estimate, one reason for more type I errors might be that markets are sometimes defined too narrowly. In phase 2, however, it appears that the EC has become too lenient, since type II errors increase during this investigation phase compared to pre-reform. The existence of political and firm lobbying might be a possible explanation. Third, according to our rent-reversion regressions, remedies are not effective before and only very loosely effective after the reform. Some outright clearances are seen by the market as good news for the rivals, possibly

indicating the cost of type II errors by the EC. Only prohibitions achieve full rent-reversion, however, we can estimate their effect only pre-reform since only two mergers were blocked post-reform. Given the undisputed effectiveness of this merger policy tool compared to remedies, it appears that the EC blocks too few mergers. Finally, we measure significant deterrence effects pre- and post-reform. Pre-reform, these are achieved mostly via phase 1 remedies and prohibitions which is in line with these being the most effective merger control tools. Post-reform it appears that withdrawals/abortions substitute for the role of prohibitions. Our robustness checks support two of our main identification assumptions: to identify anti-competitive mergers via the rivals' change in profits seems to be a good approximation as confirmed by the regressions from the sample of purely horizontal mergers, where this identification strategy is more likely to hold. Moreover, to identify the introduction of the reform with the date of its legal implementation in May 2004 seems to be key for understanding the change in policy brought about by the reform.

In conclusion, the introduction of the ECMR 04 seems to have changed European merger policy. Yet, in terms of effectiveness along our four dimensions we paint a mixed picture. While, on the one hand, decisions are based on a more economic analysis and we observe fewer type II errors than before, we also find that the increased focus on remedies was only partially successful and cannot replace the policy tool of straight prohibitions, which solve both the competitive concerns raised by the concentration and deter future anti-competitive mergers. Clearly, this policy shift was not only the product of the reform, foremost, it might be the persistent reaction to the substantial shock and political climate which originated from the Court of First Instance's reverses of three prominent cases in the early 2000s. Yet, an approach to merger control that is more clearly based on economic principles does not necessarily mean abandoning the use of prohibitions, as shown by US antitrust authorities that are far less hesitant to block mergers than their European counterpart. The belief that remedies are a more sophisticated and cleaner instrument to almost surgically appraise merger cases seems misplaced. Thus, according to our analysis, while some of the changes brought about by the reform seem to

go in the right direction, the positive impact on the efficiency of European merger control is dampened especially by the fact that DG Comp deprives itself of its most powerful tool: prohibitions.

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Appendix

Quantifying the Effect of a Merger and Merger Decision

The estimation of the impact of a merger and merger decision proceeds in several steps. First, we estimate a market model for each firm, which allows us to simulate the counterfactual scenario of what would have happened if the merger had not occurred. Using this information, we then calculate the cumulative abnormal rents generated by the merger or merger decision over an event window spanning several days around the relevant dates. We then aggregate the cumulative abnormal returns for the merging firms and their rivals, to obtain a merger-specific information. Finally, we assume that market participants can - to a certain degree - foresee the merger decisions, which is priced in the stock of firms around the relevant event. Hence, to obtain a more precise measure of the competitive effect of the merger and merger decision, we correct for these market expectations.

The Market Model

Define $R_{i,j}$ as the return of firm i at date j and $R_{market,i,j}$ as the market return index of the branch of firm i . The market model predicts that the daily return of a commodity i is proportional to the market index at any given point in time t . Formally: $R_{i,t} = \alpha + \beta R_{market,t} + \varepsilon_{i,t}$.³⁶ We can then calibrate the coefficients of this model for all firms $i = 1, \dots, N$ over a time period of 240 trading days, namely the period from 290 to 50 days prior to the announcement of the merger.³⁷ Letting the estimation window end 50

³⁶For the superiority of a market model over a constant mean return model in capturing abnormal returns see MacKinlay (1997) or Schwert (1981).

³⁷For some cases the market model could not be reliably estimated in this period due to data limitations. In these cases the estimations window was shifted to 530 - 290 days prior to announcement.

days before the announcement (that is, the date on which the financial press wrote about the proposed transaction) should yield unbiased estimates of the market model's coefficients and, hence, the 'normal' firms' return, which is our counterfactual and that is given by: $\hat{R}_{i,t} = \hat{\alpha} + \hat{\beta}R_{market,t}$.

The Event Windows

The event windows are the time intervals around the dates of the relevant events (e.g. merger or merger decision), during which new information hits the market. In the absence of any information leakages, these windows can be reduced to the event day. The larger the expectations that some information was leaked to the market prior to the event, the larger the window should be. Hence, the length of these windows is critical to the event study's ability to capture the profitability effects: if the window is too small, the effect might not be wholly captured, whereas too large a window could dilute the result.³⁸ To account for the structurally different circumstances of the various events we consider, we use both a *long* as well as a *short window*. The long window is the interval $[t - 50, t + 5]$ (where t designates the date of the event), the short window is $[t - 5, t + 5]$.

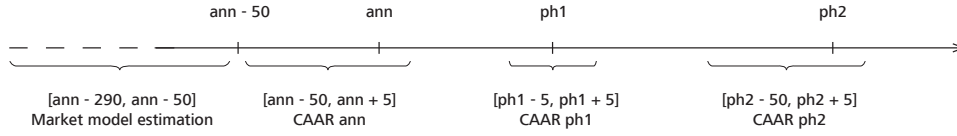
For the announcement and the phase 2 decision, we employ the long window. In both cases information leakages could occur substantially earlier than the date of the event in question. Rumors of mergers often circulate for weeks before definitive signs reach the financial press. The same holds for an in-depth merger investigation in phase 2, during which the Commission often contacts competitors and customers of the merging firms during its assessment and information is likely to leak to the market.³⁹ These prolonged processes could easily reduce uncertainty and allow the concerned parties to adjust their anticipations.

³⁸Issues concerning the length of event windows and their ability to capture the effect of regulation are more thoroughly discussed in Duso, Gugler, and Yurtoglu (2010).

³⁹The EC has a time-frame of 90 working days between phase 1 and phase 2 decisions.

The phase 1 investigation, on the other hand, lasts only 25 working days and is conducted internally by DG Comp. Furthermore, a substantial part of this relatively short time is utilized for the appraisal of administrative issues. We therefore assume that information leakages to the market occur no earlier than 5 days before the decision and that the stock prices adjust in a short window around the decision. The event windows are schematically depicted in figure 3.

Figure 3: Timeline of the events



Aggregating the Abnormal Returns

The abnormal return of firm i at date j is defined as

$$AR_{i,j} = R_{i,j} - \hat{R}_{i,j}.$$

The cumulative abnormal returns (CAR) are then obtained by summing up the abnormal returns over the event window (t_1, t_2) :

$$CAR_i(t_1, t_2) = \sum_{t=t_1}^{t_2} AR_{i,t}.$$

These CARs measure the profitability impact of a combination at the firm-level. Measuring firm-level effects has the advantage of allowing for asymmetric externalities of a merger.⁴⁰ While we allow for asymmetric externalities at the

⁴⁰It is an empirically well-documented phenomenon that merger targets usually experience stock market gains, whereas buyers often lose. Likewise, the externalities on rivals need not be evenly distributed as the degree of competition among firms might vary.

firm-level, the definition of an anti-competitive merger has to be done at the aggregate level, since what matters for the policy is the impact of the merger on the overall consumer surplus. Hence, to obtain a measure of the total impact of a merger, we aggregate the merging firms' as well as rivals' CARs at the merger level by using the relative market value of each firm as a weight.⁴¹ The cumulative average abnormal returns (CAAR) at event e (announcement, decision) for firms f ($f = M$ for merging firms and C for their competitors) in merger j are then given by

$$CAAR_{fj}^e = \frac{\sum_{i=1}^{N_{fj}} CAR_i^e mv_i}{\sum_{i=1}^{N_{fj}} mv_i} \quad e = ann, dec \quad f = M, C \quad j = 1, \dots, 326 \quad (7)$$

where N_{fj} denotes the number of merging firm or rivals for merger j and mv_i is the market value of firm i . The CAARs, as an aggregate measure of the implications of a merger, are used to classify pro- and anti-competitive mergers and serve in the probability of intervention estimation.

Correcting for Expectations

We assume that market participants can to a certain degree anticipate the decisions of DG Comp, but that there is no perfect foresight: If the market could perfectly foresee the actions of the EC, there would be no significant stock reactions around the decision dates. The fact that there are significant deviations from the market trend when news of a decision reaches the concerned market participants can be interpreted as evidence in favor of our assumption. Furthermore, the existence of prohibitions contradicts perfect foresight: if managers could perfectly foresee the actions of DG Comp, mergers that end up being blocked would not have been attempted in the first place, nor would there have been significant reactions in response to their announcements.

⁴¹The idea of a 'firm portfolio' weighted by market values is owed to Schwert (1981).

Using the past merger control history and the knowledge of the structural characteristics of a proposed merger, firms can form a prior of how likely it is that DG Comp will intervene. This means that the observed abnormal returns around the event dates do not measure the full effect but are the expectation-adjusted abnormal returns, which take into account that the combination might not go through or be subjected to remedies. Since we assume that the market's assessment reveals the competitive nature of a combination, we would like to remove this adjustment of expectations to obtain the market assessment in absence of merger control.

If expectations are rational, the expected value of the EC's decision is:⁴²

$$E[\Pi^{dec}] = \rho \Pi^{action} + (1 - \rho) \Pi^{clear} \quad (8)$$

where $\Pi^{action}(\Pi^{clear})$ denotes the merger's profitability in case of an action (a clearance) and ρ is the probability of an action. The observed abnormal returns around the announcement (Π^{ann}) therefore are equal to the real effect (Π^{ann*}) plus the expected value of the EC's final decision ($E[\Pi^{dec}]$). Assuming that an intervention by DG Comp destroys the anti-competitive rents generated by a combination ($\Pi^{action} = -\Pi^{ann*}$) in their full extent ($\Pi^{dec*} = \Pi^{ann*}$),⁴³ and that a clearance has no further effect on the market ($\Pi^{clear} = 0$), the impact of a merger can be written as:

$$\Pi^{ann} = \Pi^{ann*} + E[\Pi^{dec}] = \Pi^{ann*} + \rho \underbrace{\Pi^{action}}_{=-\Pi^{ann*}} + (1 - \rho) \underbrace{\Pi^{clear}}_{=0} \Leftrightarrow \Pi^{ann*} = \frac{\Pi^{ann}}{1 - \rho} \quad (9)$$

Similarly, the effect that we measure around the decision (Π^{dec}) is an update of the market's beliefs concerning that particular decision and, hence, the

⁴²Note that, to ease notation, we eliminate the subscript for the firms' types ($f = M$ for merging firms and $f = C$ for competitors) and the merger j .

⁴³We realize that this assumption might be questioned, but it is necessary for probability correction and seems less arbitrary than ex ante assuming a certain nonzero degree of rent reversal.

difference between the merger's competitive effect and the market expectations of the commission decision's effect.⁴⁴

$$\Pi^{dec} = \Pi^{dec*} + E[\Pi^{dec}] = \Pi^{dec*} + \rho\Pi^{action} + (1 - \rho)\Pi^{clear} \Leftrightarrow \Pi^{dec*} = \frac{\Pi^{dec}}{1 - \rho}$$

If a case goes into phase 2, the market will again update its beliefs about remedies.⁴⁵ The effect around the phase 1 decision accounts for the adjustment of market expectations to the new state of beliefs, the sum of both decision effects captures the total impact of the EC's decision. The real effect of the decision is then given by

$$\Pi^{dec*} = \frac{\Pi^{P1} + \Pi^{P2}}{1 - \rho}$$

where Π^{P1} (Π^{P2}) is the measured effect around the phase 1 (phase 2) decision date.

Combining the equations for the decisions yields

$$\Pi^{dec*} = \begin{cases} \frac{\Pi^{dec}}{1 - \rho} & \text{if phase 1 case} \\ \frac{\Pi^{P1} + \Pi^{P2}}{1 - \rho} & \text{if phase 2 case} \end{cases} . \quad (10)$$

Thus, to account for expectations, we need to estimate the ex ante likelihood of an intervention for every merger j (ρ_j) and correct the CAARs measured around the announcement (Π_{fj}^A) and the decision (Π_{fj}^D) of that merger according to equations (9) and (10). This refinement improves the precision of the estimate of the market competitive assessment of a merger.

⁴⁴If the market had perfect foresight, we would measure only white noise around the decision. The surprise value of the decision is due to the private information generated during the legal proceedings.

⁴⁵The probability of a clearance subject to conditions and obligations is much higher for phase 2 cases than for phase 1 cases; a blocking is possible only after a phase 2 investigation.

Investigating Transatlantic Merger Policy Convergence*

Florian Szücs[†]

Abstract We propose a framework to examine tendencies of convergence in the jurisdictional patterns of the American FTC and the European Commission. Based on a sample of 493 merger cases scrutinized by one of these agencies in the 1999 - 2007 period, we calibrate logit models of the probability of intervening in a merger for both jurisdictions and use them to predict the decisions of the respectively other agency. The results point to an increasing harmonization of merger policies and corroborate the theoretical appraisal, that the 2004 reform of EU merger law constituted a step towards the US system.

Introduction

The Federal Trade Commission (FTC) and the Directorate General for Competition (DG Competition) are among the most important regulatory authorities in merger control worldwide. Their verdicts on merger cases can shape global markets to a substantial degree and the scope of their jurisdictional competence extends far beyond national (or communal) borders. And yet these two institutions differ greatly in history, method and aim: whereas US merger control - and antitrust in general - looks back on a long history, for the longest

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part of which it was shaped and developed in the spirit of a pro-competitive doctrine with the goal of ensuring dynamic and efficient market structures,¹ the common European competition authority is a relatively young institution whose goals, like those of its predecessors, are more pluralistic in nature. While ensuring competitive markets features prominently among DG Competition's objectives, issues of market integration, a distinct distrust for concentrated markets and market foreclosure as well as political motives also play a role.²

This article attempts to evaluate empirically if and to what degree convergence of US and European merger policies took place between 1999 and 2007. This time period is a particularly interesting subject for an investigation of this kind, because it includes data prior to and after the 2004 reform of European merger law (ECMR04),³ which led to an increased use of economic analysis in merger review (the 'more economic approach') and was interpreted as a step towards US policy (for example Verouden, Bengtsson, and Albaek (2004); Coppi and Walker (2004); Bergman, Coate, Jakobsson, and Ulrick (2010)). We explicitly address this issue by checking for changes in the coefficients of the EU model after May 2004, including post-reform dummies in all regressions and comparing post-reform European decisions to the jurisdictional patterns of DG Competition and the FTC prior to the reform.

While the subject of convergence of merger policies has been discussed in great detail from a theoretical point of view by legal and economic scholars as well as practitioners, empirical evidence on the issue is sparse. The goal

¹Kovacic and Shapiro (2000) give an overview of US antitrust from the Sherman Act to Post-Chicago economic thinking, Mueller (1997) describes the historical development until the nineties and surveys many empirical studies.

²Coppi and Walker (2004) and Shenefield (2004) discuss the different aims of US and common European competition law, Schwartz (1993) sketches the development of national European merger policies up to the common European Community Merger Regulation (ECMR) of 1989, Cini and McGowan (1998) continue from there. The 2004 reform of European merger policy is reviewed and evaluated in Lyons (2004) and Duso, Gugler, and Szücs (2010). Bergman, Jakobsson, and Razo (2005) and Duso, Neven, and Röller (2007) provide empirical studies on the determinants of an intervention by DG Competition.

³Regulation No 139/2004 of 20 January 2004 on the control of concentrations between undertakings.

of this article is thus to quantitatively reassess the theoretical findings on the convergence of transatlantic merger policies by empirical analysis. To do this, a database containing 493 merger cases scrutinized by the FTC or DG Competition (or, in rare cases, both) during the period from January 1999 to December 2007 has been assembled to investigate whether their jurisdictions have become increasingly similar. The notion of similarity is particularized by employing two empirical measures of convergence developed in the literature.

Merger policy in the US is exercised by the FTC and its sister agency, the Department of Justice (DoJ). The division of merger cases between the two agencies is effected on an (informal) industry basis. Since data on DoJ cases are available only at an aggregate level, they cannot be used in the analysis undertaken in this article, which requires detailed case information. Therefore, this article's conclusions, strictly speaking, only apply to US merger policy as practiced by the FTC. However, the US Horizontal Merger Guidelines,⁴ which constitute the basis of the merger assessment by both agencies and offer detailed standard procedures, have been developed and published jointly by the FTC and the DoJ since 1992. Therefore, issues of merger policy convergence within the US are of much lesser concern than tendencies of convergence with different juridical systems. It might thus be argued that the US cases contained in the sample employed here can - at least when contrasted with a system of competition policy, the dissimilarity of which arguably dwarves intra-US discrepancies - be regarded as representative of US merger control.⁵

The findings of this empirical investigation are very much in line with those of the theoretical literature: While there can be no talk of perfect convergence, the progress made in terms of harmonization is substantial. After a period of increasing dissonance until 2002/03, both jurisdictional systems seem to have gradually improved their understanding of how merger control works on the

⁴Department of Justice & FTC, Horizontal Merger Guidelines (1992, rev. 1997) and Department of Justice & FTC, Horizontal Merger Guidelines (2010). See Shapiro (2010) for a discussion of the evolution of the US Horizontal Merger Guidelines.

⁵Potential biases due to the lack of DoJ data in the sample are addressed econometrically in and .

other side of the Atlantic. In particular, the 2004 reform of European merger law seems to have been a substantial step towards US merger policy.

The remainder of this article is structured as follows: Section summarizes the history of policy convergence and reviews the relevant (economics and political science) literature, section presents the data, section the methodology employed. Results are presented in section . Section concludes.

Historical overview and literature

The general notion of policy convergence in competition policy has been a subject of discussion for quite a while now. Scherer (1994, 1997) discusses the general tendency of worldwide competition policy to approach pro-competitive doctrines over the course of the twentieth century. The idea that competition policy should ensure competitive markets seems self-evident from today's point of view, but at the beginning of the twentieth century only the US was actively prosecuting monopolies and cartels (for example Kovacic and Shapiro (2000)). In Europe and large parts of the rest of the world cartels were thought to dampen the impact of business cycles (for example Audretsch (1989)), while monopolies were deemed necessary to operate on efficient production scales and compete internationally. The change towards a pro-competitive doctrine came about after WW2, in the second half of the last century. In this sense, there has undeniably been a lot of progress in the harmonization of competition law. In this article, however, we take for granted that competition authorities in general (political or other motives aside) pursue the goal of ensuring competitive markets and instead focus on the convergence of their jurisdictional patterns, that is, their decision to intervene in certain mergers and to clear others.

More recent tendencies concerning the convergence and potential conflicts in international competition law are considered in Calvani (2004). Calvani examines the evolution of cooperation among competition authorities in the period from 1990 to 2004, gives examples for areas of convergence and remaining discrepancies and concludes in favor of harmonization. While Niels and

Ten Kate (2004) focus on normative differences between the US and the EU approach to competition law (economic vs. legalistic approach, treatment of dominant firms etc.), Coppi and Walker (2004) discuss technical differences in the evaluation of mergers (market definitions, econometric techniques, concentration measures, dominance vs. market power; the perceived 'unilateral effects gap' of pre-2004 European competition law is discussed in particular depth). Shenefield (2004) argues in a similar vein, highlighting the role of different objectives in US and EU antitrust. All of them concede that while some differences remain, tendencies of 'soft' convergence are undeniable.

Cooper, Froeb, O'Brien, and Vita (2005) detect differing views on vertical policy on both sides of the Atlantic: While DG Competition takes vertical agreements very seriously, US agencies have a more lenient regard of them.⁶ In contrast, opinions on horizontal combinations (as expressed in the respective 'horizontal merger guidelines': FTC (2010), DG Competition (2004)) seem to be rather similar in both competition authorities (for example Verouden, Bengtsson, and Albaek (2004)). Horlick and Meyer (1995) argue that competition policy convergence is especially noticable in merger control, because it is in the interest of all countries concerned. In other areas of competition policy, for example the regulation of subsidies and tariffs, international consent might be harder to achieve due to conflicting national interests.

The topic of policy convergence is also discussed in the political science literature. Even though this literature focuses mainly on tendencies of global convergence of environmental and labor policies (for example Busch and Jørgens (2005)), the driving forces for convergence identified can be assessed with a regard to competition policy. The most frequently cited drivers for policy convergence (for example Drezner (2001)) are: i) a race-to-bottom (that is, complete deregulation) mechanism fueled by economic pressure, ii) various forms of institutionalism and iii) an epistemic communities approach.⁷ While

⁶To control for this possible area of divergence, the analysis is repeated in a subsample excluding non-horizontal mergers as a robustness check. See section .

⁷The epistemic communities approach is related to the elite consensus and the world society approaches, which are not discussed separately in this brief overview. Holzinger and

mechanism i) (a 'race of deregulation' among nations) seems implausible in competition policy, ii) arguably does play a role in the harmonization of global antitrust: institutions like the International Competition Network (ICN) or the World Trade Organization (WTO) certainly have their part in levelling competition policies around the globe. Van Waarden and Drahos (2002) propose, that the epistemic communities approach is most suited for explaining the convergence of competition policies. This approach postulates that the emergence of a community of legally trained officials - with similar epistemic beliefs - provides a channel for the international diffusion of ideas, methods and practices, which - in turn - cause convergence. See Haas (1992) for an introduction to the topic.

To make the rather vague concept of 'convergence of policies' more tangible, several concepts of convergence have been developed in the literature (see for example Sala-i-Martin (1996a,b); Heichel, Pape, and Sommerer (2005)). The concepts of convergence relevant for the purposes of this article are discussed in .

Data

The data used in this analysis were created by combining two datasets on mergers, one containing EU cases, one containing US cases. The EU dataset comprises 310 merger cases handled by DG Competition in the time period from 1990 to 2007. The US dataset contains 420 FTC cases, which were assessed between 1999 and 2009.⁸ The analysis is restricted to the overlap period of 1999 to 2007, for which there are observations on both agencies, allowing for a direct comparison. 226 EU cases and 267 US cases (493 cases

Knill (2005) review the causes of policy convergence in the political science literature in greater detail.

⁸Actually, the US sample contains about 50 cases decided prior to 1999 all of which are interventions, since data on clearances were not available for that period. These observations are thus used for the calibration of logit models only.

in total) fall into this time period; the remaining observations are used for the calibration of the binary decision models only.

The EU cases were collected from DG Competition's homepage.⁹ Going through the decisions, the merging parties and the outcome of the investigation was recorded. Similarly, the US cases were obtained from the data available on the FTC homepage,¹⁰ again identifying merging parties and outcome. The *Hoover's*¹¹ database was used to determine the primary competitors in each merger case. The companies (merging parties and their competitors from US and EU cases) were then linked to the Thomson Reuters *Worldscope* database,¹² from which data on market values, R&D expenditures, dividends, industry classification, geographic location and other structural characteristics were downloaded. Tables 1 and 2 summarize the dataset.¹³

⁹<http://ec.europa.eu/competition/mergers/cases/>.

¹⁰<http://www.ftc.gov/bc/hsr/index.shtm>.

¹¹<http://www.hoovers.com/>.

¹²http://thomsonreuters.com/products_services/financial/financial_products/products_az/worldscope_fundamentals.

¹³A total of 235 merger cases of our sample, 83 DG Competition and 152 FTC cases, were decided in the time periods from 1990-1998 or 2008-2009 respectively and are thus not included in the summary statistics. The characteristics of these cases do not significantly differ from those reported in tables 1 and 2.

Table 1: *Summary Statistics of EU cases*

Year	Obs.	Action	MV M.	MV R.	R&D M.	R&D R.	Div. M.	Div. R.	Horiz.	Duration
1999	20	0.40	13.35	15.43	10.88	13.15	26.75	90.02	0.70	69
2000	26	0.73	14.35	17.94	12.27	14.95	43.60	145.00	0.62	119
2001	30	0.47	12.66	16.50	11.23	14.40	33.59	115.39	0.77	98
2002	28	0.50	15.87	17.43	11.41	13.66	34.72	81.30	0.61	63
2003	19	0.63	16.41	17.61	12.44	14.34	28.06	132.74	0.79	82
2004	22	0.59	16.40	17.91	12.71	14.41	32.68	112.73	0.68	84
2005	26	0.65	16.04	18.58	11.42	13.93	36.82	106.25	0.62	66
2006	26	0.50	17.06	18.96	11.89	14.34	32.64	115.10	0.65	68
2007	29	0.55	16.69	18.91	12.17	13.98	37.82	85.77	0.66	68

Table 2: *Summary Statistics of US cases*

Year	Obs.	Action	MV M.	MV R.	R&D M.	R&D R.	Div. M.	Div. R.	Horiz.	Duration
1999	46	0.52	17.38	19.18	11.99	14.87	15.98	79.68	0.78	151
2000	38	0.53	17.26	19.16	12.37	15.28	19.09	95.32	0.66	147
2001	35	0.46	17.38	18.99	12.48	14.60	25.36	112.10	0.86	214
2002	24	0.42	17.40	20.01	13.08	16.24	27.60	143.75	0.67	162
2003	23	0.43	16.87	19.30	12.98	16.08	24.78	141.86	0.78	147
2004	18	0.50	17.11	19.61	12.70	15.36	50.21	128.62	0.61	147
2005	23	0.48	17.24	18.84	11.89	14.73	27.81	157.73	0.78	120
2006	22	0.45	16.74	19.80	12.78	16.12	23.71	140.64	0.64	144
2007	39	0.28	16.81	19.82	12.73	16.04	15.87	130.74	0.82	87

Variable Description: Obs.: Number of mergers in the respective year; Action: Fraction of mergers in which an action (remedy or blocking) was taken; MV M. (MV R.): Average combined market value of the merging parties (rivals); R&D M. (R&D R.): Average combined R&D spending by the merging parties (rivals) [Note: All MV and R&D values are reported as logs of 1,000 USD]; Div. M. (Div. R.): Sum of dividend payments per share by the merging parties (rivals) relative to earnings; Horiz.: Fraction of horizontal mergers; Duration: Average duration from merger announcement to decision in days. For the sake of brevity, some variables have been excluded from the tables.

The average rate of intervention (*Action*, the terms 'action' and 'intervention' are used interchangeably throughout this article) in the sample is 49.9% and higher in EU cases (56%) than in US cases (42%).¹⁴ The main difference between the US and EU subsamples lies in the size of the mergers: the market values and R&D expenditures of merging parties and rivals are higher in US mergers than in European mergers, whereas European merging parties seem to pay (relatively) higher dividends. US and EU cases therefore are, to a degree, structurally different. This issue is explicitly addressed in section , where the analysis is restricted to propensity-score matched subsamples to control for the possibility of a sample selection bias. The majority of cases (about 2/3 in the EU and slightly less than 3/4 in the US) can be classified as 'horizontal', that is, mergers between competing firms.¹⁵ The duration is defined as the number of days between the announcement (the date at which the financial press first wrote about the combination) and the publication of the decision by the agency.¹⁶ Since the duration is on average substantially longer in the US (the EU enforces a strict time schedule), we use a variable containing the quartiles of the regime-specific duration in the logit regressions to increase comparability across regimes.

The variables not reported in tables 1 and 2 are the approximated market shares of merging parties and their biggest competitor, industry dummies and dummies for the involvement of EU/US firms in a merger. We use the ratio of sales by merging parties and competitors as a proxy for the merging parties' market share (average EU: 31%, average US: 17%); similarly, the share of

¹⁴Less than 5% of the cases in the sample were either prohibited by DG Competition or abandoned after the FTC succeeded in obtaining a preliminary injunction in court. Since the focus of this article lies on the decision to intervene in a merger and not on the choice or adequateness of the measure of intervention, these cases are not distinguished from other remedies. In section we run a robustness check excluding these cases.

¹⁵Since the FTC does not publish its economic assessment, this measure is based on industry classification codes.

¹⁶This includes the timeframe between the merger becoming known and the date of its notification to the authority, as well as weekends and festivities and therefore does not strictly correspond to the duration of the investigation.

sales by the largest competitors (EU: 47%, US: 41%) is a further indicator of market concentration. The decomposition into industry branches is similar in both jurisdictions: A majority of mergers in manufacturing (EU: 62%, US: 71%) and transport & communications (EU: 23%, US: 10%), along with some in trade (EU: 7%, US: 9%) and services (EU: 7%, US: 15%) are complemented by relatively few mergers in the finance (EU: 3%, US: 1%) and mining & construction (EU: 6%, US: 2%) industries. Since these dummies are based on industry classification variables, they are neither exclusive nor exhaustive. The means of the dummies for involvement of US/EU companies in a merger are high in both jurisdictions with more US firms being involved in US mergers and vice versa.

Methodology

This section outlines this article's approach to policy convergence, discusses the concepts and measures of convergence employed and presents some details concerning the propensity score matching procedure we use to obtain a homogenous subsample.

Approach

The keystone of this inquiry in policy convergence are logit models, which emulate the decisions to either intervene or clear a merger case by the FTC and DG Competition. The dependent variable, *action* (equal to zero if the case was cleared, equal to one if it was either remedied or blocked), is regressed on possible determinants: dummies for horizontal mergers, the involvement of EU/US companies, industry and political dummies, as well as the market values, R&D expenditures and dividends of both merging parties and rivals in the merger, two variables proxying for the market shares of merging parties and the biggest competitor and a variable indicating the duration of the investigation. To calibrate these models, the whole sample of merger cases is used

(310 observations for the EU model, 420 for the US model). The models are presented in section .

These logit models are then used to predict the likelihood of an intervention in cases handled by the respectively other competition authority, the counterfactual decision. That is, the US logit model is used to predict the probability with which the FTC would have intervened in EU cases and vice versa. The degree of compliance between the actual decision in a merger case by one competition authority and the counterfactual decision (predicted by the logit model) of the other is - evaluated over time - the basis of the analysis of jurisdictional convergence.

Do the model coefficients change over time?

Pooling all observations into a single model for each jurisdiction means implicitly assuming that both jurisdictions can be regarded as static constructs over the whole sample period. While continuity and predictability are desirable traits of a merger control authority, this is a strong assumption which has to be justified. We address this issue by first identifying points in time at which structural breaks in competition policy could plausibly occur, then estimating the jurisdictional models in the non-overlapping partitions thus defined and using a generalized Hausman specification test to compare these restricted models to the unrestricted model. The null hypothesis of the Hausman test is, that there is no systematic difference in the model coefficients. If this is rejected, separate models will have to be estimated for the periods in question.

In the US sample, the most plausible points to test for structural breaks (following Bergman, Coate, Jakobsson, and Ulrick (2010)) are potential regime changes due to changing FTC chairmen. The sample period includes the terms of Timothy Muris and Deborah Platt Majoras.¹⁷ Timothy Muris' term (June

¹⁷The term of Robert Pitofsky (1995 - 2001) is partially contained in the sample as well. Although we do not have sufficient data to test for regime changes during his chairmanship, Coate and Ulrick (2006) find no statistically significant difference in enforcement between the terms of Robert Pitofsky and Timothy Muris and conclude that '[...] merger enforcement policy has remained relatively stable during the 1996 to 2003 time period'.

2001 - April 2004) contains 100 observations and 124 further cases were handled during Deborah Platt Majoras' term from May 2004 to May 2008. We thus estimate two logit models, restricted to these terms of office, in the same specification¹⁸ as reported in , and compare them to the unrestricted model, estimated using all US observations. Most of the coefficients are similar in size and equal in sign across all three models (differences occur mainly in industry dummies) and the Hausman test does not reject the null hypotheses of equal coefficients to the unrestricted model during the Muris term ($p = 0.45$) and during the Majoras term ($p = 0.61$). This continuity in US merger control in our sample may well be due to the fact that the whole sample falls between the 1997 revision of merger guidelines and the publication of the 2010 merger guidelines. Thus, while US merger policy certainly was not perfectly static, there are no obvious discontinuities in our data on US merger control, allowing us to pool all US observations.¹⁹ While the results of the restricted models (which are not significantly different from the unrestricted model) are omitted for the sake of brevity, the model containing all US cases is reported in .

The most obvious potential regime change in our data on European merger control is due to the major changes in European merger legislation brought about by the ECMR04. We thus estimate two models of EU jurisdiction, one prior to and one after the reform in May 2004, and compare them to the EU model containing the whole sample period. Again, a Hausman test is employed to compare the model coefficients. While the Hausman test does not reject the null hypothesis in the pre-reform period ($p = 0.56$), there is a significant difference in coefficients when comparing the post-reform period to the whole sample of EU cases ($p = 0.04$). Thus, the perceived major impact of the 2004 reform on European merger (for example Drauz and Reynolds (2003)) law is reflected in the data and will have to be made allowance for by using separate

¹⁸Minus the political control variables, which are not necessary in the subsamples considered here.

¹⁹Similarly, Leary (2002) concludes in favor of essential stability of US merger policy in the 1980ies and 90ies.

models of EU jurisdiction for the periods prior to and after it. The two EU models are reported in as well.

Propensity score matching

As mentioned in section , the observations in the US and EU subsamples are to a certain degree different with regard to the characteristics we observe: while market values and R&D spending are higher for US merging firms and rivals in the sample, EU merging firms pay higher dividends. Industry dummies differ across the subsamples as well. This could be either due to differences in the kind of mergers that are being investigated in the US and Europe, or due to the limitation of our sample to FTC cases for a lack of DoJ data. We address this issue explicitly via the use of propensity score matching.

Propensity score matching (PSM) was developed by Rosenbaum (1983) and is used to reduce the bias due to sample selection.²⁰ The concept is frequently applied to the experimental design in the medical sciences: when trying to single out the effect of a treatment, the subjects of the control group should be as similar as possible to those of the treated group. The PSM algorithm provides a measure of similarness based on a set of covariates.

In the context of our analysis, being 'treated' means being handled by one competition authority (the problem is symmetric) and the PSM algorithm will select a sample of cases of the other competition authority. The sample thus obtained will be more uniform with respect to the specified covariates. Notice that in this setting there is not one 'treated' and one 'control' group, but actually two treated groups. We thus divert the algorithm from its originally intended use (singling out a causal effect between treatment and control groups) and employ it to increase the similarity of two subsamples.

The covariates used to calculate the propensity score are: dummies for horizontal mergers, US and EU firms and industry dummies, as well as market values, R&D spending, dividends and a variable indicating the duration of the investigation. We use a version of the PSM algorithm developed for Stata

²⁰For an application to mergers see Weichselbaumer (2008).

by Leuven and Sianesi (2003), modified to find unique best matches within the same year and removing them from the pool after matching. Thus, the algorithm calculates the matrix of propensity scores between all EU and US cases in a given year, finds the best match (the lowest propensity score) and removes the two cases thus matched from the pool. Then the second best match is selected and so on. Calibrating the algorithm such, that after the matching procedure the average difference in propensity scores of the yearly matches (the average structural difference of cases in the subsample, based on the above covariates) stays around 5%, determines the amount of matches per year at 14. This yields 28 merger cases per year from 1999 - 2007, producing a subsample of 252 cases in total.

All of the results presented in the next section will be iterated in this subsample. While the whole sample contains the maximum amount of cases available to us and thus allows the most surveying picture of the two jurisdictional systems, the matched subsample is designed to eliminate effects that arise due to differences in the types of cases the two authorities handle.

Concepts and Measures of Convergence

At least four types of convergence can be found in the relevant literature: β -, γ -, δ - and σ -convergence. While β -convergence is mostly used in the literature on economic growth (catching-up processes) and γ -convergence is concerned with the mobility of countries in country-rankings at different points in time, the concepts of δ - and σ -convergence can be readily applied to the purposes of this analysis.

σ -convergence, named after the algebraic notation for standard deviation, occurs when the dispersion of two time series decreases over time. In the growth literature, this means that the absolute difference between the GDP levels under investigation declines. Applied to competition policy, the concept is a bit more complex. We will say that two competition policies (as manifested in the patterns of jurisdiction by the respective agencies) exhibit decreasing dispersion and therefore σ -convergence if the absolute difference of their pre-

dictions decreases over time. That is, two agencies σ -converge if their decisions on the same cases become more similar over time. Since the subsample of cases which were scrutinized by both the FTC and DG Competition (the 'overlap' of jurisdictions) is too small for statistical analysis, the following workaround will be applied: Using logit models calibrated to the jurisdictions of the FTC and DG Competition, we will predict the ex ante likelihood of an intervention for all cases from the point of view of both agencies. Thus, we simulate a scenario in which all cases in our sample were handled by both competition authorities. This makes the verification of σ -convergence straightforward: σ -convergence occurs in our sample if the absolute difference of predictions by the two models decreases over time; that is, if the 'variance of jurisdiction' of the two agencies diminishes.

Formally, the absolute difference of predictions in merger i is defined as

$$\sigma_i = |P_i^{US}(action) - P_i^{EU}(action)|, \quad (1)$$

where $P_i^j(action)$ ($j = US, EU$) is the predicted probability of an intervention by agency j in merger i . σ -convergence occurs if $\lim_{i \rightarrow \infty} \sigma_i = 0$, but in a finite sample we will conclude in favor of σ -convergence if the average σ_i decreases over time, while an increase would imply divergence.

δ -convergence, named after the algebraic notation for distance in topological space, is defined in Heichel, Pape, and Sommerer (2005) as 'decreasing distance of policies towards an exemplary model'. This point of reference, the 'exemplary model', could be a frontrunner country or a purely abstract ideal state of policy. To investigate the existence of δ -convergence in our sample, we are going to use the model of one agency to predict the decisions of the other (the counterfactual decision) and assess to which degree the predictions are correct.²¹

We calculate the prediction errors of the model of agency j in predicting the decisions of the other agency, $-j$, in merger i as

$$\delta_i^j = |action_i^{-j} - P_i^j(action)|, \quad (2)$$

²¹When talking of the 'correctness' of decisions, we always refer to 'agreement with the actual outcome' and not correctness with respect to some welfare criterion.

where $action_i^{-j}$ is the decision of agency $-j$ in merger i (equal to one if an intervention took place in merger i and zero otherwise) and P_i^j is defined as above. Based on this, we investigate whether the actual decisions by one competition authority converge towards the counterfactual decisions of the other competition authority, that is, if $\lim_{i \rightarrow \infty} \delta_i^j = 0$. We will conclude that agency $-j$ δ -converges towards agency j , if the agreement of decisions by $-j$ and predictions by the model of j increases over time, that is, if δ_i^j decreases.

If the performance of one logit model is constant with regard to these measures, the merger policy of the agency, whose decisions are being predicted by the model has remained unchanged in comparison with the merger policy embodied in the model. If, conversely, a model gets better at predicting the other competition authority's decisions over time, the jurisdiction of the predicted entity has become increasingly similar to the modelled entity and has thus δ -converged towards the jurisdiction captured in the model (a catching-up process). If a model gets worse, the jurisdiction of the other entity has become more dissimilar. If the predictions of both models improve, we observe bilateral tendencies of harmonization: both jurisdictions δ -converge towards each other.

Results

Logit Models

To simulate the patterns of jurisdiction of the two competition authorities, three logit models are calibrated using the subsamples of US cases, EU cases prior to ECMR04 and EU cases post-ECMR04. For the purpose of the estimation of these models, all available cases (including those beyond the 1999 - 2007 window) are used, providing 420 observations for the US model and 212 and 98 observations for the EU models respectively. The dependent variable in all models is *action*, the decision to either clear a merger or intervene. Table 3 contains the results.

Table 3: Logit Models: Probability of Intervention

	EU model pre	EU model post	US model
Horizontal mergers	0.737 (0.554)	-1.266 (0.815)	-0.030 (0.429)
US dummy	-1.255* (0.644)	-0.124 (0.882)	-1.620*** (0.600)
EU dummy	-1.702*** (0.663)	0.185 (0.825)	-0.355 (0.611)
Market value merging	0.281*** (0.082)	0.164 (0.193)	0.000 (0.136)
Market value rivals	0.252*** (0.078)	0.221 (0.225)	0.265 (0.182)
R&D merging	-0.384*** (0.133)	-0.625*** (0.216)	-0.180 (0.132)
R&D rivals	-0.118 (0.127)	-0.239 (0.209)	-0.529*** (0.163)
Dividends merging	0.012 (0.008)	0.027* (0.016)	0.006 (0.007)
Dividends rivals	0.001 (0.004)	0.012* (0.007)	0.016*** (0.003)
Duration of proceedings	2.225*** (0.335)	1.976*** (0.478)	2.691*** (0.306)
Share of sales biggest rival	-0.265 (1.374)	2.843 (2.673)	2.735* (1.431)
Share of sales merging	-0.274 (1.422)	1.767 (2.573)	0.292 (1.654)
Bush administration	-0.624 (0.660)		-1.785*** (0.527)
Observations	212	98	420
Pseudo R^2	0.56	0.51	0.68
Correctly Classified	87%	89%	91%

Standard errors in parentheses, the symbols ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively. Regression includes controls for the political environment (EU Commissioners and US presidents) and industry effects.

Prior to the 2004 reform of European merger law, the main drivers for an intervention by DG Competition were the market values of both merging parties (proxying for the importance of the transaction) and rivals (proxying for market size). As predicted by economic theory, R&D-intensive markets received less regulatory attention. Political factors seem to have played an important role as well: firms originating from the EU or from the US were treated preferably in comparison to firms from other parts of the world, EU firms even more so than US firms. Finally, a longer duration of the investigation increased the likelihood of an intervention.²²

Post-reform, the determinants for an intervention of DG Competition change substantially: among the significant results, only those for R&D-intensive markets and duration remain. The effect of the duration of proceedings slightly decreases in comparison to the pre-reform model, possibly reflecting the increased focus on phase 1 remedies. In contrast to the pre-reform model, the dividends of both merging parties and rivals (proxying for the maturity and profitability of the industry) now increase the likelihood of an action, while lobbying by US and EU firms no longer has an impact on the decision.

The FTC is less likely to intervene in mergers involving US-based firms. While higher industry profits - as proxied by rivals' dividends - increase the probability of an intervention, R&D spending of rivals decreases it. The effects of the duration of an investigation is stronger than in the EU models. The share of industry sales held by the largest competitor indicates the degree of industry concentration and increases the likelihood of an action. Whereas the Bush administration had no significant effect on European merger control,²³ it significantly facilitated merging activity in the US.

²²We tackled the possible endogeneity of the duration of investigation variable by specifying instrumental variable models treating it as endogenous and using regime variables (commissioners/chairmen) as excluded exogenous variables. Employing both two-stage least squares and maximum-likelihood estimation, we could not reject the null hypothesis of exogeneity in any of the models.

²³The coefficient in the post reform period - ranging from May 2004 to December 2007 - could not be estimated due to perfect collinearity.

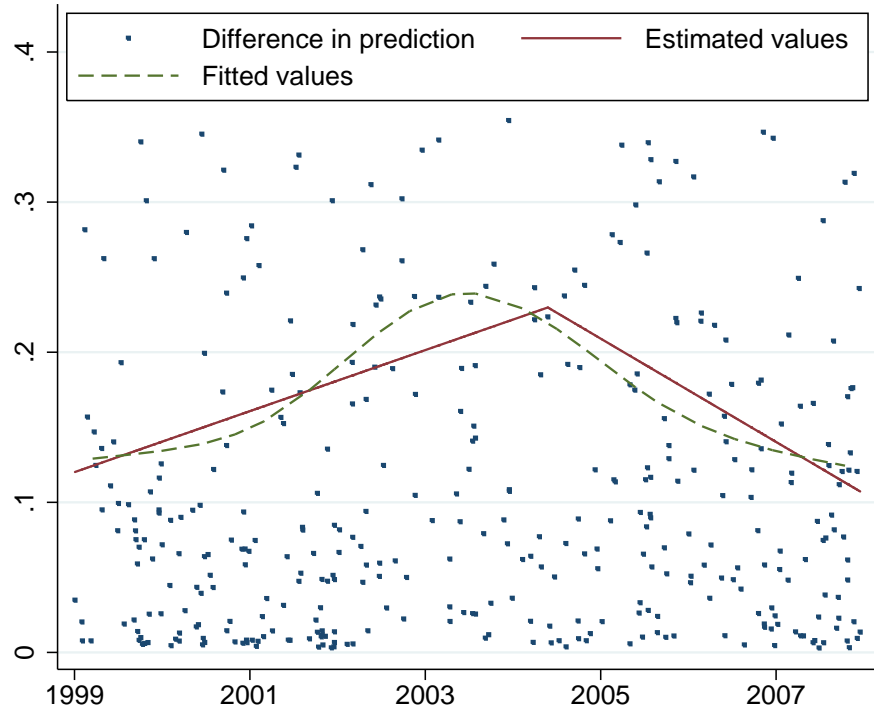
While the size and significance of coefficients vary across the models, the signs of the six continuous economic variables (market values, R&D spending and dividends of both merging parties and rivals), which constitute the cornerstone of economic analysis, do not change: in all specifications, the likelihood of an intervention increases with market values and dividends (size and maturity/profitability) and is decreased by R&D spending (innovations, market dynamics). The share of sales by merging parties in relation to their rivals, on the other hand, is insignificant in all three specifications. Either the agencies are not influenced by simplistic market-share considerations, or sales ratios are a too vague proxy for them.

The goodness of fit measures are very good in all models: R^2 s of 56% and 51% in the EU models and 68% in the US model permit the correct classification of 87%, 89% and 91% of observations respectively. The fact that the US model fares better in terms of both measures indicates that the FTC's jurisdiction can be better explained by circumstantial information (as employed in the model) than the decisions by the European Commission. Furthermore, the predictability of European merger control seems to have decreased in the post-ECMR04 period. In spite of the smaller sample size, the R^2 of the post-reform model is 5% lower. Thus the FTC's decisions appear to be more transparent than those of DG Competition.

Convergence

In this section we use the logit models estimated in to predict probabilities of intervention and apply the measures discussed in to the predictions. All the figures include a scatterplot containing the data-points (top and bottom percentile excluded for visual clarity), a restricted cubic smoothing spline of the data (as implemented in Cox (2007), intended as a visual aid) and a plot of the predictions of a linear model, regressing the data on a general time-trend and a time-trend restricted to the period after May 2004 (the post-reform period). The coefficients of the time-trend regressions are reported in table 4.

Figure 1: Difference in predictions

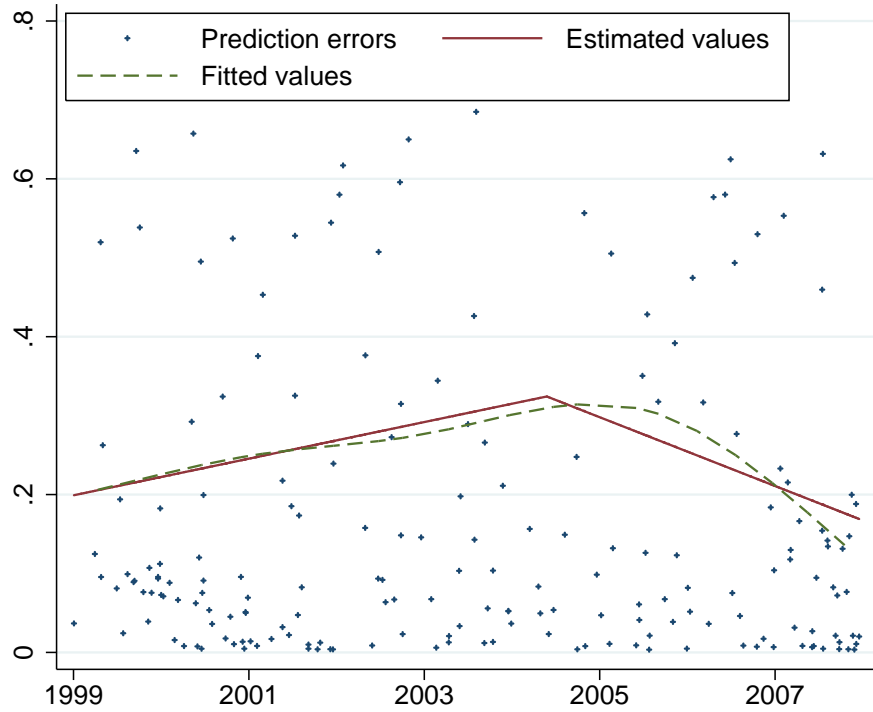


Difference in predictions by EU and US models on all cases in sample. For clarity's sake, the scatterplots omit the top and bottom percentiles of values.

Figure 1 reports the difference in the predictions by the EU and US models, the measure of σ -convergence, for each merger case in the sample in the 1999 - 2007 period (493 observations).

The predictions of the two models differ by about 12% on average at the beginning of the sample period. This difference increases to about 24% until 2002/03 and then steadily declines from 2004 on. While the yearly maximum occurs in 2002, the spline suggests a peak during the course of 2003. Thus both measures place the peak of incongruity before the coming-into-force of ECMR04 in May 2004, suggesting that the changes in merger law might have been anticipated by legal practice. The plotted regression shows a significant and positive trend for the pre-reform period. The coefficient (see table 4) suggests a yearly increase in the difference of predictions by 2.6%. Post-reform,

Figure 2: Prediction errors of EU model



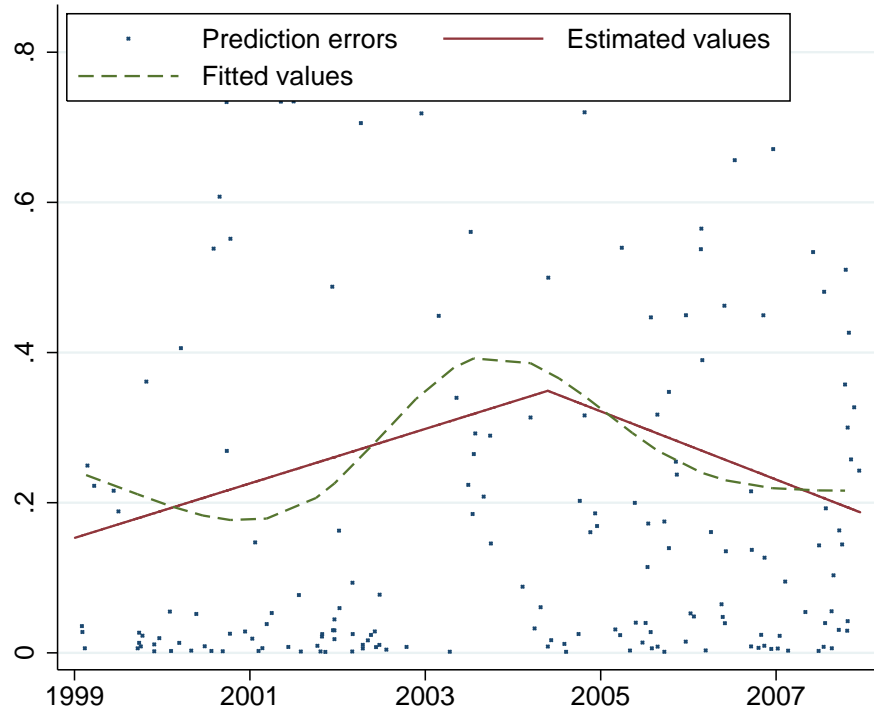
Difference between EU model predictions of US cases and actual outcomes. For clarity's sake, the scatterplots omit the top and bottom percentiles of values.

this trend is superseded by a negative trend of - 5.2%, significant at the 1% level. The net effect of the two trends is a yearly reduction of the difference by 2.6%. Since the decline of this measure is equivalent to a reduction in the variance between the two underlying time-series, figure 1 indicates strong tendencies of σ -convergence between EU and US merger policies from 2004 until 2007. More than half of the difference in predictions vanishes during that period.²⁴

Figures 2 and 3 show the prediction errors of both models in predicting decisions by the respectively other agency.

²⁴The negative trend continues in 2008 and 2009. 2008/09 data are not included in the graph, because they contain only US observations.

Figure 3: Prediction errors of US model



Difference between US model predictions of EU cases and actual outcomes. For clarity's sake, the scatterplots omit the top and bottom percentiles of values.

Table 4: Time Trends

	Difference	EU model error	US model error
Year	0.026*** (0.008)	0.029* (0.016)	0.037* (0.019)
Post-reform year	-0.052*** (0.013)	-0.058** (0.026)	-0.063** (0.032)
Observations	493	267	226

Robust standard errors in parentheses, the symbols ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.

While the two fitted splines exhibit different dynamics (slow increase until 2005 followed by a slump in the EU model; initial decrease, followed by a sharp increase and again decrease from 2004 on in the US model), both substantially decrease in the post-reform period. The regression plots show a similar picture: after a period of increasing prediction errors prior to the reform (EU model: +2.9% annually, US model: +3.7% annually, both significant at the 10% level), the trend turns negative afterwards (EU: -5.8% annually, US: -6.3% annually, both significant at the 5% level).

Thus, while both jurisdictions started out on relatively similar ground in 1999 (with average prediction errors of about 0.2 each), they became increasingly alien to one another up to the years 2003/2004 (the maximum average prediction error of the US model, 0.46, occurs in 2003, that of the EU model, 0.40, in 2004). After this point, strong tendencies of δ -convergence can be observed until the end of the sample period, when the yearly average prediction errors of the EU model reach their minimum (0.17). Yearly average prediction errors of the US model are slightly lower in 2001 (0.18) than in 2007 (0.19), but 2007 is the minimum among post-reform observations.

Summing up, all of the above measures seem to agree, that after a peak of incongruity in transatlantic merger control between 2002 and 2004, substantial convergence of jurisdictional patterns occurred in the second half of the sample period, presumably in relation with the institutional changes in European merger law triggered by ECMR04. In the post-reform period, both the prediction errors and the differences in predictions of both models decline, signifying δ - and σ -convergence respectively. This is a first empirical indication, that the 2004 reform of European merger law was indeed a step towards the US system. We will try to further substantiate this finding in section .

Convergence post-ECMR04

As shown above, the EU merger policy reform of 2004 constituted a major shift in the direction of European merger control (see also Duso, Gugler, and Szücs (2010)). This shift has been interpreted as a step towards US merger

policy on theoretical and legal grounds (for example Verouden, Bengtsson, and Albaek (2004); Rusu (2007); Bergman, Coate, Jakobsson, and Ulrick (2010)).

To empirically investigate this hypothesis the following framework is proposed: The logit models of the decisions of both FTC and DG Competition are estimated with all available data up to May 2004 (the month in which ECMR04 came into force) and then used to predict the decision of DG Competition after the reform.²⁵ Calculating the prediction errors of both models with respect to the actual EU decisions will allow us to infer the proximity of the actual jurisdiction to the US and the EU models.

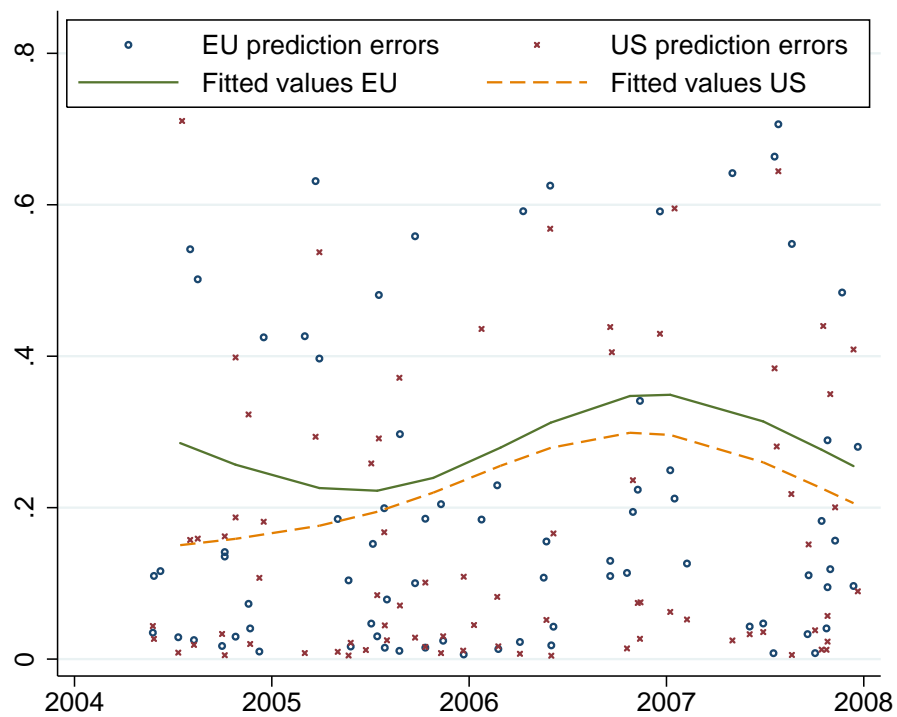
Figure 4 reports the prediction errors of the (pre May 2004) EU and US logit models in forecasting EU decisions in the June 2004 - December 2007 period (98 observations).

As illustrated by the fitted curves, the prediction errors of the EU model are, on average, larger than the prediction errors of the US model during the whole period. This suggests that post-ECMR04 European merger control is better understood by the pre-ECMR04 FTC than by pre-ECMR04 DG Competition. The difference between the average prediction errors of the EU model (0.28) and the US model (0.23) is highly significant ($p = 0.01$). Unsurprisingly, the FTC model is also - again, $p = 0.01$ - better at predicting its own post-ECMR04 decisions. This corroborates the conjecture that the evolution of US merger control in the sample period is a continuous one, whereas the ECMR04 significantly altered European merger policy.

Since the findings presented in this section are based on sparse data (98 observations), covering a timeframe of only 3 (and a half) years, their nature is more indicative than conclusive. Still, the results of this empirical inquiry are in accord with the widespread opinion among practitioners and scholars that the 2004 EU merger policy reform constituted a step towards US merger policy.

²⁵The results of the two logit models are similar to those in section and are not separately reported here.

Figure 4: Predicting post-reform EU decisions



Prediction errors when using pre-ECMR04 models to predict post-ECMR04 EU cases. For clarity's sake, the scatterplots omit the top and bottom percentiles of values.

Convergence in Matched Subsample

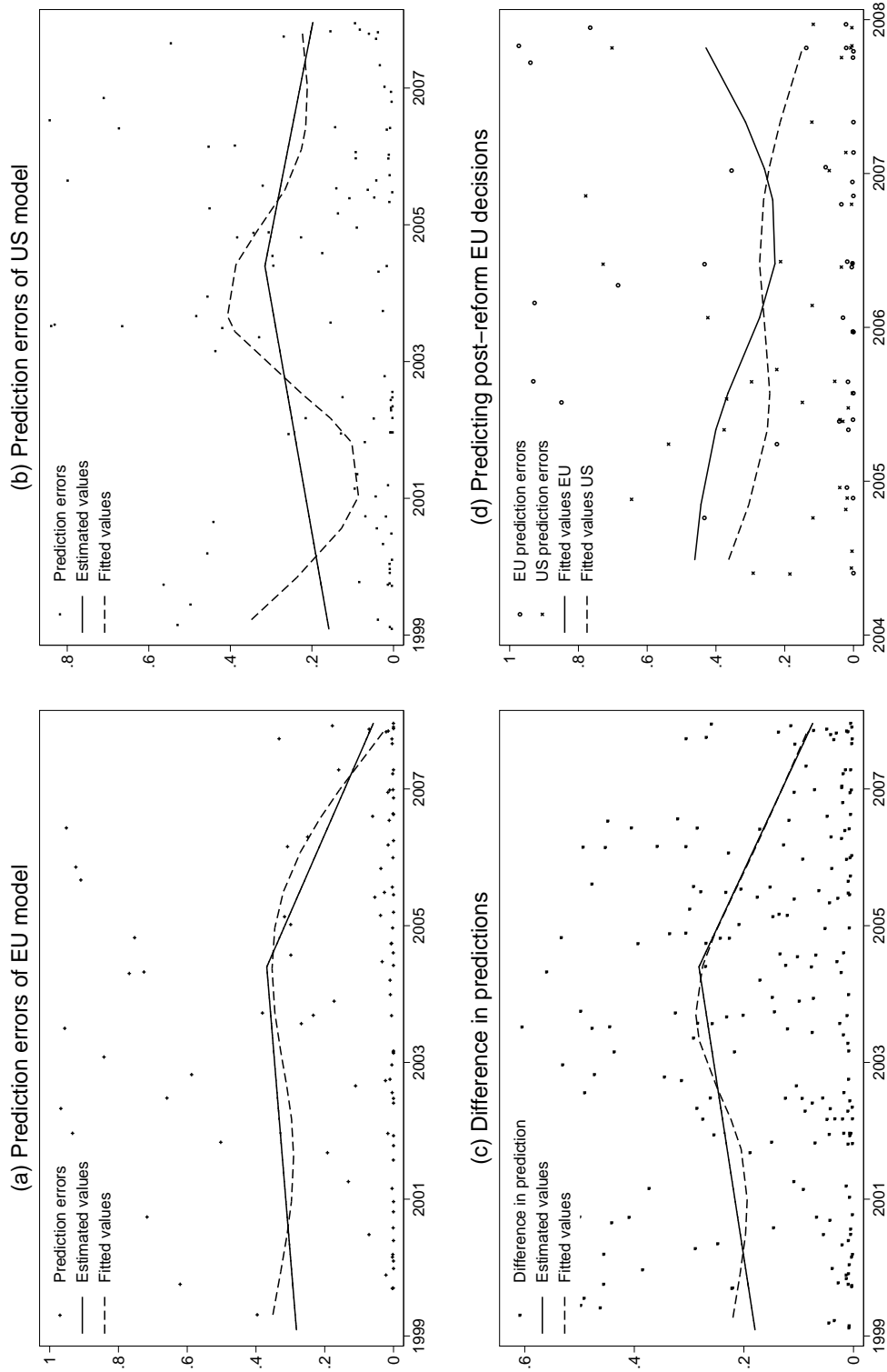
We repeat the exercise conducted in the previous section in a propensity-score matched subsample (containing 252 observations) to account for possible structural heterogeneity of the merger cases in our sample. We estimate the same logit models (except for some industry and political dummies which had to be dropped by the regression algorithm in the smaller sample) as in table 3 and employ the same measures of convergence as before. The results are reported in figure 5.

Essentially, all the conclusions inferred in the whole sample can be maintained in face of the subsample-results. The two graphs containing the prediction errors of the EU and the US model (panels (a) and (b)) strongly resemble those reported in section , with on average higher prediction errors in the subsample. This is due to the decreased accuracy of the logit models calibrated in the smaller sample. The smaller sample size also affects the significance of the time-trend regressions: the positive pre-reform trends in prediction errors are insignificant in both models, the same applies to the negative post-reform trend in the US model. In the EU model, the post-reform trend remains significantly (5% level) negative and corresponds to an annual decrease of 10.7% in prediction errors.

The difference in predictions (panel (c)) shows less pre-reform volatility and higher differences than in the whole sample, with the average difference hovering around 20%. The increasing trend is significant at the 10% level (+2.6% per anno). The negative time-trend after the reform is even stronger (-7.1% per anno) than in the whole sample and significant at the 1% level, inducing a net effect of -4.5% per year after the reform. The predictions of the pre-reform US model still outperform those of the corresponding EU model (panel (d)). The average difference of the predictions by both models increases from 0.05 to 0.11, but the significance is slightly reduced ($p = 0.04$) due to the smaller sample size ($n = 51$).

Overall, the gain from controlling for differences between US and European mergers seems to be exceeded by the loss from dropping half of the sample:

Figure 5: Convergence in propensity-score matched subsample



Panel (a) and (b) report the prediction errors of the EU (US) model in forecasting US (EU) investigation outcomes. Panel (c) reports the difference in predictions by both models when predicting all cases in the sample. Panel (d) reports the errors of EU and US models calibrated to pre-ECMR04 data in predicting post-ECMR04 EU cases. For clarity's sake, all scatterplots omit the top and bottom percentiles of values.

the quality of the logit models suffers from the small amount of observations in the subsample and, since none of the results essentially change, the structural differences of mergers in the sample are apparently not overly important.

Robustness Checks

This section briefly discusses the results of a number of robustness checks which were performed on the data.

Horizontal mergers

Dropping all non-horizontal mergers reduces the EU sample to 201 and the US sample to 310 observations. While the difference in prediction errors after ECMR04 becomes insignificant ($p = 0.11$, due to the smaller sample size, the actual difference remains), all other results reported in are robust to this.

Prohibited and abandoned mergers

About 4% of cases in the EU sample were blocked by DG Competition. Similarly, 4% of the US sample mergers were abandoned by the parties after the FTC obtained a preliminary injunction in court. Since these cases were considered strongly anticompetitive by the respective competition authorities, they are potential outliers in comparison with the rest of the sample. Dropping these cases slightly improves the significances of the prediction error time-trends, while the significance of the post-reform difference in predictions is marginally reduced ($p = 0.02$). All other results remain unchanged.

Calibrate using only sample data

The logit models employed are calibrated using some observations on US and EU merger control outside the sample period 1999-2007. Dropping those observations improves some of the time-trend significances, but reduces the significance of the difference in post-ECMR prediction errors ($p = 0.08$).

Control for industry dissimilarities

Some of the industry dummies (reported in section) differ substantially among the EU and US sample. The dummies for the finance and transport & communications industries have significantly higher means in the EU sample, since in the US these industries are routinely regulated by the DoJ. Dropping all observations in these industries reduces the EU sample size to 169 and the US sample size to 238. Estimating in these samples slightly reduces the significance of the difference in post-ECMR04 predictions errors ($p = 0.04$), while leaving other results qualitatively unchanged.

Conclusion

The degree of coherence of globally effective merger policies, such as those exercised by the EU and the US, is highly relevant from both the firms' point of view as well as that of economic efficiency. Since policies are never perfectly static constructs - merger policy in particular has seen radical changes in the last decades -, not only the degree of coherence matters, but also whether the policies converge or diverge. We attempt to address the questions raised by these observations in an empirical framework by applying notions of convergence developed in the growth and political science literature to merger control.

While merger policy in the US evolved continuously during the sample period (models restricted to the terms of individual chairmen are not significantly different from the unrestricted model), EU merger policy experienced a major shift induced by the reform of European merger law in 2004. In line with the theoretical literature on the topic, the empirical models presented here indicate that the effect of the reform was a step towards greater coherence with US merger law. In particular, the logit model calibrated with US data predicts the outcomes of post-reform EU merger cases significantly better than the EU model, suggesting the European merger control after the reform is relatively nearer to US merger control than to the pre-reform EU system. All estimated

time trends of prediction errors by the two models turn significantly negative in the periods after the reform, indicating increasing consent of the underlying jurisdictions.

Shortly before the reform, however, the coherence of merger policies seems to be at its lowest level in the sample period. One plausible explanation for this finding is that the European Court of First Instance overruled three of the Commission's decisions to block mergers during 2002 and criticized the economic analyses conducted in these cases. These politically embarrassing overrulings may well have caused unsettling repercussions in EU merger control, which were only subdued by the increased focus on economic analysis introduced by the reform.

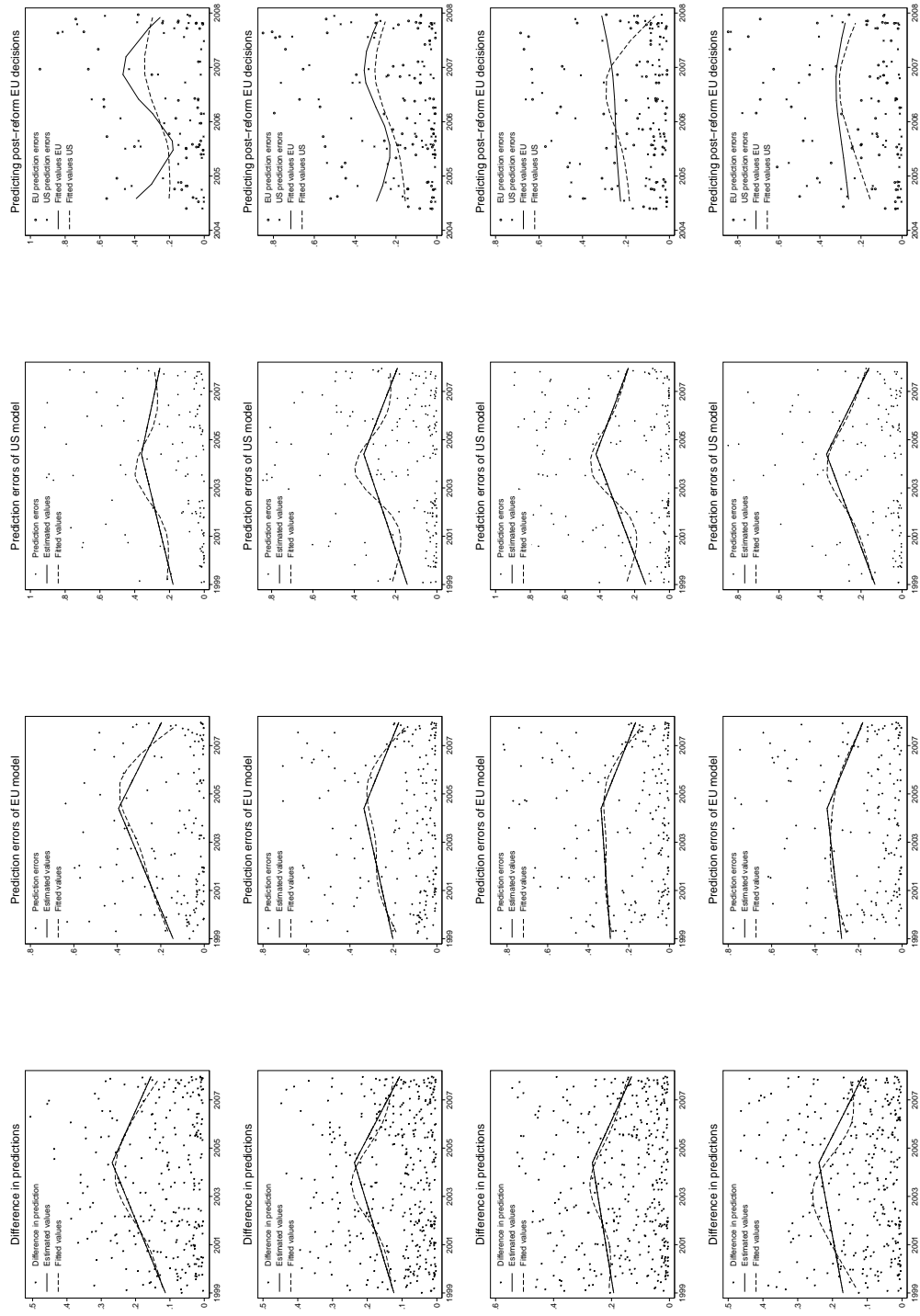
The coherence of the two systems is largest at the end of the sample period: the differences in the predictions by both models as well as the prediction errors of the models in forecasting decisions by the respectively other agency are at very low levels and exhibit a downward trend. It thus seems that substantial convergence was achieved in the last few years and that we may hope that this trend is still at work.

Robustness Checks Results

Figure 6 contains the graphical results when the robustness checks discussed in section are applied. The first row shows the four main graphs (difference in model predictions, EU model prediction errors, US model prediction errors and post-reform prediction errors) when only horizontal mergers are considered. In the second row, prohibited and abandoned mergers are removed from the sample. The third row reports the results obtained, when only sample data (that is, observations between 1999 and 2007) are used to calibrate all models. Finally, in the forth row the finance, transport and communications sectors were dropped from the sample.

As discussed in section , all main results are quite robust to these checks.

Figure 6: Robustness Checks



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M&A and R&D: Asymmetric Effects on Acquirers and Targets?*

Florian Szücs[†]

Abstract We evaluate the impact of M&A activity on the growth of R&D spending and R&D intensity of 265 acquiring firms and 133 merger targets in the time period ranging from 1990 to 2009. We use a range of matching techniques to construct separate control groups for acquirers and targets and use appropriate difference-in-difference estimation methods to single out the causal effect of mergers on R&D growth and intensity. We find a significant reduction of R&D efforts by both acquirers and targets in the periods after the merger, pointing to a decrease of the incentive to innovate.

Introduction

The present paper continues to investigate the nexus between corporate mergers and the incentive of firms to allocate resources to innovation activities and hopes to overcome some of the shortcomings of previous efforts on the same issue. This paper's main contribution is probably the explicit differentiation of effects on acquirers and targets. Previous studies have included both acquiring firms and merger targets in their analysis (Cassiman et al. (2005); Ornaghi (2009)), but effects were measured in a pooled setting, due to either small sample sizes or the inability to differentiate the correct roles.

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Furthermore, earlier studies on the subject matter were usually either of limited geographical scope (Bertrand (2009); Stiebale and Reize (2011)) or restricted to certain industries (Bertrand and Zitouna (2008); Ornaghi (2009)). The database utilized in this study contains firms from most major industrialized nations, active in numerous different industries. Thus we hope to overcome any industry or country-specific effects and provide a surveying picture of the phenomena in question.

Restructuring R&D activities is a protracted affair that can take a number of years to complete. Therefore the explanatory power of short-term studies on the topic is limited. To account for the relevant time horizon, we use balance sheet data from up to 6 periods after the acquisition year. Time windows of $[t + 1, t + 6]$ years after the acquisition year t allow us to check for drawn-out restructuring efforts after it. While we use pre-merger data (period $t - 1$) in the estimation of the ex-ante probability to merge, data from the merger period t are excluded from the analysis to avoid the measurement of consolidation effects of the combination.

The goal of this paper is to contribute to the empirical discussion on the relationship between mergers and the *incentive* to conduct innovative efforts. We therefore analyze the effect of mergers on two measures of R&D inputs: the growth of R&D expenditures and R&D intensity, defined as the ratio of R&D expenditures over sales. By making R&D inputs instead of R&D outputs (patents, new products) the focus of the analysis, we examine the firms' willingness to invest in innovation instead of their success in attaining it. Thus, questions about synergies and changes in the efficiency of research are not addressed by this paper. However, Hagedoorn and Cloudt (2003) show that measures of R&D inputs and outputs are highly correlated and conclude that there is no major systemic disparity between them.

A much-discussed issue in the evaluation of non-experimental data concerns the issues of missing data and self-selection. The basic problem is that, in a non-experimental setting, self-selection into the 'treated' group cannot be ruled out and thus receiving the treatment might be non-random, confounding the measurement of the causal effect of treatment. Therefore great care has to

be taken in the construction of an appropriate control group as well as in the specification of the empirical strategy to derive reliable results.

In this respect, we follow the suggestion of Blundell and Costa Dias (2000) and combine matching techniques with difference-in-difference estimation. Three different matching techniques (nearest-neighbor matching, Mahalanobis metric matching, caliper matching) and a very rich pool of potential control observations are used in the construction of the control groups, in which the difference-in-difference estimation is then performed. In each case, separate control groups are constructed for acquirers and targets to account for firm heterogeneity due to their roles in the transaction. Estimation results are reported in all three samples thus obtained.

When estimating the ex-ante probability to be involved in a merger, we find similar determinants for acquirers and targets: high values of R&D intensity, total assets and employees increase both the probability of being an acquirer or a target. The firms' profitability, on the other hand, raises the probability of being an acquirer and decreases that of being a target: acquirers are significantly more and targets are significantly less profitable than the average firm in the sample.

In the early periods after the merger, acquirers do not differ significantly from the control group in terms of R&D growth. We find some negative growth effects from $t + 2$ to $t + 5$, though only the effect in $t + 5$ is significant in all specifications. The R&D growth of merger targets, conversely, drops sharply relative to the control group in all periods from $t + 1$ to $t + 5$ after the acquisition and all specifications.

The effects on R&D intensity are negative as well: while both groups start out at very high levels of R&D intensity (the average pre-merger R&D intensity of acquirers is between 6 and 7%, that of targets is approximately 8%) this changes significantly after the acquisition. We measure highly significant negative effects on acquirers in all periods ($t + 2$ to $t + 6$) and all specifications suggesting a monotonic reduction of R&D intensity amounting to more than 3 percentage points six periods after the acquisition. The effects on merger targets also point to a monotonic decrease of R&D intensity after the merger.

The coefficients suggest an average reduction in the R&D intensity of approximately 4 percentage points six periods after the combination.

These observations are consistent with the interpretation that merging firms are very innovative prior to the merger, but that in the post-merger period the incentive to invest in innovation is substantially decreased. This points to a reduction in competitive pressure achieved by the merger, either through the elimination of an innovative competitor or through an advantage over competitors entailed by the advance of the acquirer's technological portfolio. In either way, the M&A activity in this sample has, on average, entailed a significant reduction of the innovative efforts of the parties involved.

Literature

The literature on the effects of mergers on innovation is a large and fast-growing field, since it receives a lot of attention from both economics and management scholars. To keep this section concise, we will focus on rather recent contributions in the economics tradition, thereby neglecting earlier studies and corporate governance considerations.

An article closely related to this one is the study by Ornaghi (2009), which analyzes the effect of 27 mergers in the pharmaceutical industry on various measures of R&D inputs and outputs. A combination of propensity score matching and difference-in-difference estimation and, alternatively, a measure of technological relatedness is used to address issues of endogeneity. When estimating the effects on acquirers and targets in a pooled setting, Ornaghi finds a decrease in innovative efforts after mergers. Stiebale and Reize (2011) report similar findings from a sample of 304 German merger targets and explicitly control for structural zeros in reported R&D values (see section and Kleinknecht (1987)).

Desyllas and Hughes (2010) analyze a sample of 2624 acquirers in high-tech industries using a similar empirical strategy. They find that the R&D intensity of an acquiring firm decreases in the period after a merger ($t + 1$) but increases again in the $t + 3$ -period. R&D productivity is not significantly

affected. They also find evidence in favour of the view, that mergers between technologically-related firms perform better than mergers between firms that differ greatly with respect to their knowledge bases. This argument is also advanced by Cassiman et al. (2005), who distinguish between technological and market-relatedness and use a detailed sample of 31 mergers. Contrariwise to Desyllas and Hughes (2010), they find that technologically complementary (substitutive) firms increase (decrease) their R&D level after the acquisition. Moreover, effects on R&D efficiency are more advantageous in complementary mergers.

Ahuja and Katila (2001) distinguish technological acquisitions (i.e. acquisitions that are primarily technologically motivated) from nontechnological acquisitions. Their sample consists of 72 large chemical companies, engaging in 534 acquisitions. Their analysis reveals that nontechnological acquisitions do not significantly influence innovative output. While technological acquisitions generally improve innovative output, the extent of the improvement depends on the technological relatedness of the two firms in a nonlinear fashion. Cloudt et al. (2006) extend the approach of Ahuja and Katila to four high-tech industries. Whereas their findings with respect to technological acquisitions are largely compatible with those of Ahuja and Katila, they find that nontechnological acquisitions have a negative impact on innovative performance after the merger.

Studies that find increases in R&D activity after mergers include Bertrand (2009) and Stiebale (2010). Using a sample of 123 French acquisition targets in crossborder mergers and a combination of propensity score and difference-in-difference methods, Bertrand (2009) finds that R&D budgets have significantly increased three years after the acquisition. Stiebale (2010) focuses on acquirers (324 firms) and finds that their R&D intensity significantly increases after the merger.

As can be seen from this brief overview, empirical studies on the effect of mergers on R&D efforts either find a positive, negative or ambiguous relationship. Therefore, no clear-cut empirical conclusions have emerged so far. Still, most reviews of the literature (an excellent survey is provided by Veugelers

(2006)) conclude in favour of a weak, negative relationship between M&A and R&D.

Data & Empirical Strategy

The dataset used in this study was created by joining datasets of mergers that were notified to either the European Commission (EC) or the Federal Trade Commission (FTC) between 1990 and 2009. These cases were reported to the respective regulatory authority by companies from 25 different nations¹ and many different product markets² and were either cleared or subjected to remedies by the authorities. The only common factor in all of these mergers is that they were significant enough to meet the notification thresholds of the EC or FTC.³ Thus the sample does not include minor asset acquisitions, which entail no significant effect on companies, but major transactions resulting in significant corporate restructuring under the scrutiny of one of the two most important antitrust jurisdictions. Some of the firms in the sample merge more than once during the observation period; to ensure that the effects of multiple mergers do not confound the results, we drop observations where not at least 4 consecutive years lie between the acquisitions.

We combine this dataset of mergers with balance-sheet data containing the R&D expenditures of the merging parties and other relevant variables. After dropping all observations, for which R&D expenditures data were not available in a time window of $[t - 1, t + 1]$ around the merger, we are left with 398 firms

¹Most of the firms involved in these mergers have their headquarters in the US, followed by Germany, France and the UK.

²38 different 2-digit SIC codes are represented in the sample. The biggest single sector is SIC 28 ('Chemicals and allied products'), which includes a quarter of all observations.

³A merger has to be notified to the FTC if the deal-value exceeds 60 million USD (as of 2010). The EC uses a combined criterion of at least 5,000 million Euro worldwide turnover and at least 250 million Euro community-wide turnover, subject to further qualifications.

(265 acquirers and 133 merger targets) for which we have full R&D data.⁴ When checking for the completeness of R&D data, all observations reporting missing R&D values were dropped. We retained companies reporting zero R&D expenditures.

This sample of merging firms was then complemented with a very large sample of potential controls, from which the relevant control groups are constructed. Since the set of potential controls is more than 50 times larger than the set of merging firms, we are confident that a sufficiently close match can be found for each treated observation. For each of these firms we downloaded time series of balance sheet data on total assets, income, total sales, total debt, number of employees and R&D expenditures from the Thomson Reuters Worldscope database. After converting all values to USD and calculating the growth rate of R&D expenditures (defined as the percentage change in R&D expenditures between two consecutive periods) as well as R&D intensities (the ratio of R&D expenditures to total sales)⁵ and profitability (the ratio of net income to total assets) for all firms in all periods, we logarithmize the total assets, sales, debt, employees and R&D expenditures variables.⁶

A first look at the resulting dataset confirms that the mergers scrutinized by the FTC and the EC are indeed significant in terms of size: the average merging firm spends over 20 times more on R&D, has over 15 times more total assets and over 10 times more employees than the average firm in the dataset. Even when controlling for size effects by comparing R&D intensities, merging firms exhibit significantly higher values. It thus appears that the average firm

⁴Notice that acquirers are overweighted in the sample. This is due to the fact that post-merger data on targets are only available if the company continues to exist after the acquisition.

⁵In some cases, R&D intensities in excess of one were found, suggesting higher R&D expenditures than sales. Since these values are not implausible per se (most of them are found in high-tech sectors like pharmaceuticals or biotechnology) they were kept in the sample. To prevent any bias in the estimation coefficients due to outliers, R&D intensity values were capped at 0.5. All results are qualitatively robust to dropping these observations.

⁶We add one to all values of zero (e.g. the R&D expenditures of non-innovative firms) before taking the logarithm.

involved in a merger, which is being scrutinized by an important competition authority, is quite different from the average firm listed on any stock market in the industrialized world. In consequence, when we want to infer the effect of merging activity on innovation efforts, not any kind of non-merging comparison group will do.

Propensity-score matching: missing data and self-selection

Studies estimating the causal effect of a treatment on a group of firms or persons receiving said treatment face the fundamental problem of not knowing, what would have happened in absence of the treatment. This is often called the problem of the missing counterfactual. If we denote (following Rosenbaum and Rubin (1983)) the outcome of observation unit i receiving treatment by r_i^1 and the outcome in absence of treatment by r_i^0 , the individual treatment effect is given by

$$\Delta_i = r_i^1 - r_i^0. \quad (1)$$

Since in reality only one of the possible outcomes is observed, we are confronted with a missing data problem in estimating the individual treatment effect. Experimental studies overcome this hurdle by randomly assigning one group of observations to treatment - the treatment group -, while another group of observations does not receive treatment, the control group. The difference in outcome between the two groups can then be attributed to the effect of the treatment and is called the average treatment effect (ATE):

$$\text{ATE}_{\text{exp}} = E(r_i^1 - r_i^0). \quad (2)$$

Non-experimental studies face the additional difficulty that an appropriate control group is often hard to come by. Since the decision to receive treatment is not randomly determined by an experimenter, but - in the case of mergers - decided by the management of the firms, the assignment to treated or control group cannot plausibly be assumed to be random. Therefore, in addition

to the missing data problem, one also faces a problem of endogeneity or self-selection, suggesting that the decision to receive treatment is caused by certain firm-specific characteristics which in turn could also influence the effect of the treatment. Not recognizing this complication could cause a systematic bias in the estimated coefficients, since effects attributed to the treatment might actually be due to other factors.

For example, as mentioned above, merging firms in this sample are much larger than the average firm; not taking this fact into account might lead us to attribute certain effects to the merger, while they actually could be a consequence of the size of the firm. It is therefore necessary to construct a control group, that has the same pre-treatment characteristics and thus the same ex-ante probability of receiving treatment (i.e. being involved in a merger as acquirer or target) as the group of merging firms. In non-experimental studies, the ATE needs to be calculated conditionally on the treated and control observations not being systematically different with respect to a vector of characteristics, c_i :

$$\text{ATE}_{\text{nonexp}} = E(r_i^1 - r_i^0 | c_i) = E(r_i^1 | c_i) - E(r_i^0 | c_i). \quad (3)$$

We thus need to artificially construct a sample, in which the decision to engage in a merger is not driven by certain firm characteristics and hence, to the largest extent possible, random. If successful, this both yields an appropriate control group for the estimation of the average treatment effect and eliminates the problem of self-selection.

Propensity-score matching: matching algorithms

The usual approach in the literature to account for the missing data and self-selection problems is to construct a control group using propensity score matching (PSM).⁷ The propensity score (Rosenbaum and Rubin (1983, 1985)) pre-

⁷Other options would be to follow an instrumental variable approach or to formulate an equation describing selection into the treatment group and estimating it jointly with the average treatment effect by using maximum likelihood methods.

dicts the probability of receiving treatment based on observable characteristics using maximum likelihood estimation. By matching treated observations to control observations based on their propensity scores one obtains two groups that do not differ systematically with respect to the observable characteristics the propensity score was calculated upon (see Rosenbaum and Rubin (1983) for the proof). PSM thus controls for the observable heterogeneity between treated and control observations.

We follow this approach by creating separate control groups for acquirers and targets using three different matching algorithms: nearest-neighbor matching within the same year, Mahalanobis metric matching within same year and 2-digit industry code as well as (global) caliper matching. The propensity scores are calculated using pre-merger (t-1) data to ensure that the merger effect does not influence the matching. Each matching method faces a trade-off between variance of the estimates (depending on the size of the control group) and bias (depending on the similarity of the control group to the treated group, i.e. the quality of the matches).⁸ The following paragraphs briefly describe the advantages and disadvantages of the three methods employed with regard to this trade-off.

Nearest-neighbor matching

Nearest-neighbor matching is probably the most intuitive matching algorithm we use and balances the trade-off between bias and variance: each merging firm is matched to exactly one non-merging firm within the same year. The match is thus the firm which is most similar to the merging firm based on the matching covariates in the year before the merger. Since every control is selected only once (matching without replacement), this yields a control group of the same size as the treated group. Matching within the same year ensures that both the treatment and the corresponding control observation refer to the same time window.

⁸Caliendo and Kopeinig (2008) and Dehejia and Wahba (2002) discuss this trade-off and the merits of different matching approaches.

Thus the nearest-neighbor matching algorithm effects a compromise with respect to the trade-off described above: having exactly one control for every treated observation ensures that the control group is not too small (variance), restricting matching to subsamples with corresponding time entries ensures that controls are sufficiently comparable to treated observations (bias).

Caliper matching

Matching to multiple controls within a caliper provides a larger control group than the two other approaches, thus alleviating concerns about the variance of the estimates. Caliper matching is implemented by matching each treated observation to the three most similar control observations, given that none of them differ by more than 0.025 from the treated observation's propensity score.⁹ Matching is performed without regard to temporal or industry subsamples; thus matches potentially are selected from different industries and/or time periods. Picking multiple matches per treatment observation provides a larger sample size for estimation.

Caliper matching results in a larger control group (since there are up to three controls per treated observation) with good matching quality (since the matches are selected from the largest possible pool). Conversely, matches are not pre-selected from appropriate categories as in the other two approaches.

Mahalanobis metric matching

The Mahalanobis metric approach places more emphasis on the bias aspect than on the variance aspect: we require the control observation to be an exact match with respect to time and industry classification. By using a full set of 2-digit SIC codes, we strongly reduce the number of available matches on the one hand, while increasing the appropriateness of the remaining matches on

⁹The caliper was determined by following the suggestion of Rosenbaum and Rubin (1985) to choose a caliper size of $c = 0.25s$, where $s = \sqrt{\frac{s_1^2 + s_0^2}{2}}$ and s_1^2 (s_0^2) refers to the the estimated variance of the propensity score in the treated (control) group.

the other.¹⁰ Since this makes the number of available matches scarce, we allow matching with replacement in this specification, i.e. the same control can be assigned to multiple treated observations.

This yields a control group that is smaller than the treated group (since matches can be recycled) and has a lower matching quality than the nearest-neighbor matching approach (since we require exact matching in two dimensions). On the other hand we know that all matches refer to the same time-frame and are within the same industry as the corresponding treatment observation.

Propensity-score matching: results

The covariates employed in the PSM algorithm are magnitudes that could potentially influence both the decision to merge and future R&D efforts, namely pre-merger R&D intensity and growth, as well as measures of pre-merger size and earnings (total assets, number of employees, profitability), debt and age of the firm. Since we expect a nonlinear relationship with the age of the firm, we also include a squared age term. The dependent variable in both regressions is a dummy, indicating if a firm was an acquirer / a target in the following period. We restrict all matching algorithms to the overlap of the distributions of the propensity scores of treated and non-treated observations, that is, we impose a common support in matching.

Table 1 reports the estimated propensity scores and shows that acquiring firms are, on average, significantly more R&D-intensive, have more assets and employees, a higher profitability and less debt than their non-merging peers. R&D growth is not a significant determinant for being an acquirer. The coefficients of R&D intensity, total assets and employees of targets are comparable to those of acquirers in terms of size and significance. While the coefficient of R&D growth is insignificant as well, there is a negative relationship between profitability and the probability of being a merger target: merger targets are,

¹⁰We observe mergers in 38 different industries over a timespan of 20 years; this divides the sample in 760 subsamples to match in.

on average, significantly less profitable than other firms.¹¹ The positive coefficient of the age of the firm along with the negative coefficient of the squared age term for both acquirers and targets suggest an inverse U-shaped relationship between age and the probability to merge: the average merging firm is neither very young nor very old.

Table 1: Propensity score estimation

	Acquirers		Targets	
R&D Intensity	3.175***	(0.325)	1.973***	(0.380)
R&D Growth	-0.020	(0.058)	-0.094	(0.080)
Total Assets	0.231***	(0.030)	0.239***	(0.036)
Employees	0.189***	(0.031)	0.132***	(0.038)
Profitability	2.101***	(0.260)	-0.573**	(0.228)
Total Debt	-0.011*	(0.007)	-0.031***	(0.007)
Age	0.030***	(0.005)	0.037***	(0.006)
Age ²	-0.000***	(0.000)	-0.001***	(0.000)
Observations	64285		64153	
Mergers	265		133	

Standard errors in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

After matching the respective control groups using the methods described above, we check whether a balanced sample was obtained by testing for systematic differences with respect to the covariates among treated and control observations in all six control groups. Table 2 reports the standardized biases before and after the respective matching procedures, as well as the reduction in bias achieved by matching.

The standardized bias ($\frac{\bar{X}_t - \bar{X}_c}{\sigma_t}$, the difference in means of treatment and control group divided by the standard deviation in the treatment group) is the bias one incurs by comparing treated to non-treated firms. As can be seen from the first column of table 2, the initial biases between merging and non-merging firms are substantial.

¹¹Gugler et al. (2003) also find that merger targets are significantly less profitable than their acquirers.

Table 2: Standardized biases of covariates before and after matching

	Nearest Neighbor		Caliper		Mahalanobis	
	Initial Bias (%)	Bias (%)	Reduction (%)	Bias (%)	Reduction (%)	Reduction (%)
Acquirers						
R&D Intensity	17.23**	2.75	84.05	3.12	83.94	4.95
R&D Growth	14.29*	0.84	94.01	2.58	81.31	10.30
Total Assets	201.40***	1.24	99.38	5.43	97.33	162.34***
Employees	183.19***	4.49	97.55	4.69	97.48	155.06***
Profitability	63.00***	2.51	96.04	0.44	99.32	59.97***
Total Debt	89.46***	4.16	95.43	4.44	95.21	57.77***
Age	66.73***	7.80	88.34	1.10	98.36	68.56***
Age ²	60.13***	6.29	89.60	0.24	99.60	61.49***
Targets						
R&D Intensity	6.93	5.48	20.86	4.51	39.15	8.69
R&D Growth	22.13*	8.87	60.09	0.40	98.20	27.36
Total Assets	163.88***	9.46	94.20	6.56	95.94	130.59***
Employees	146.26***	7.57	94.80	1.34	99.07	131.00***
Profitability	20.28*	3.48	82.77	2.25	88.91	11.54
Total Debt	60.56***	11.53	81.54	12.78	80.78	30.36**
Age	77.80***	7.08	90.82	6.97	90.83	66.76***
Age ²	68.53***	11.48	83.07	7.69	88.43	57.18***

* p < 0.1, ** p < 0.05, *** p < 0.01

Both the nearest-neighbor matching and the caliper matching algorithm largely eliminate all biases between the treated and non-treated observations. Almost all standardized biases are reduced to below 10% (the exceptions being the total debt of targets as well as the squared age of targets, where biases between 11 and 13% remain), the reduction in bias mostly exceeds 90%.¹² The degree of bias reduction is graphically illustrated in figure 1. None of the remaining biases are statistically significant. Rubin (2001) and Stuart (2010) suggest that after matching, standardized biases should not exceed 25%. This criterion is generously met by all covariates in the nearest-neighbor and caliper matched subsamples, allowing us to conclude that these two matching algorithms succeed in purging all observable heterogeneity between treatment and control group: the two groups do not differ significantly with respect to the eight covariates employed in estimation of the propensity score.

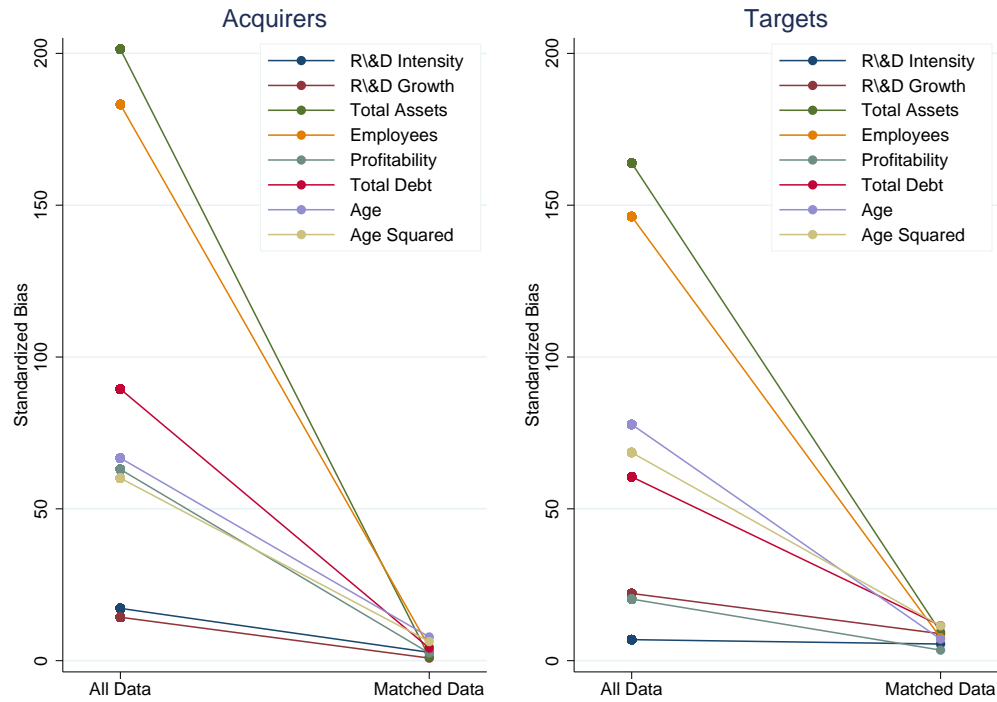
The Mahalanobis metric approach on the other hand, constrained by the large number of subsamples matching occurs in, does not succeed in balancing the sample; significant differences remain with respect to most matching covariates. Keeping this in mind, we still believe the Mahalanobis control group has some merits over the other control groups and retain it for further analysis. While it is always preferable to compare observations that are as similar as possible, a control group made up entirely from firms within the same 2-digit industry classification and referring to the same timeframe certainly is a valuable counterfactual even in absence of balanced covariate distributions.

We therefore conclude that the algorithms were successful in purging the observable heterogeneity between merging firms and non-merging firms in two out of three cases and retain the third control group for different considerations.

Finally, we check the overlap of the three matched samples (i.e. the amount of matches selected by one matching algorithm that coincide with those selected by another matching algorithm) to get an intuition of their dissimilarity. While there is a moderate amount of control observations selected by both the

¹²Lower bias reductions are achieved when i) initial biases are low (e.g. R&D intensity of targets) or ii) the variable is not a significant determinant of the propensity score and is therefore not subject to systematic balancing (e.g. R&D growth).

Figure 1: Reduction in standardized bias through nearest-neighbor matching



nearest-neighbor and the caliper algorithm (29%), the remaining overlaps are small: 5% of the controls from the nearest-neighbor and Mahalanobis samples and 5% of the control from the caliper and Mahalanobis samples coincide.

Structural zeros

Another possible bias arises due to the issue of structural zeros in accounting data on R&D spending (this is addressed in Stiebale and Reize (2011)). Many firms report zero R&D expenditures because they pursue very little or no innovative efforts and are therefore usually excluded from analysis. Yet, by excluding them one incurs a possible bias due to the selection into the group of innovative firms: it cannot be ruled out, that the effect one analyzes works systematically different on innovative firms ($R\&D > 0$) than on non-innovative

firms (R&D=0). To avoid any such bias, this sample includes both innovative and non-innovative firms: Almost 7% of merging firms in this sample report zero R&D expenditures in the merger period.

Difference-in-difference strategy

After having created the relevant control groups, we proceed to estimate the effects of mergers on the variables of interest in a difference-in-difference setting.

We construct time windows around the respective merger events and use observations of the merging firms and the relevant controls from $[t - 3, t - 1]$ and $[t + 1, t + 6]$, where t designates the period in which the combination took place. By using a set of dummies indicating whether a firm was involved in a merger one year ago, two years ago and so on, we create a merger timeline, allowing us to track the effects on innovative efforts over the time window. In the R&D intensity regression, we include further dummies for all treated observations (separately for acquirers and targets, equal to one in all periods) to control for unobservable differences between the treated and control groups. We estimate the following model

$$\begin{aligned} \text{rdint}_{ij} = & \alpha + \sum_{t=1}^6 \beta_t \text{acquirer}_{i,j-t} + \sum_{t=1}^6 \gamma_t \text{target}_{i,j-t} + \delta \text{treat_acq} \\ & + \zeta \text{treat_tar} + \eta \text{controls} + \varepsilon_{ij} \end{aligned} \quad (4)$$

The R&D intensity of firm i in year j is regressed on a set of merger dummies ranging from the year after the merger ($t = 1$) up to six years after the merger ($t = 6$) and indicating the role of the firm (acquirer or target), dummies for being an acquirer / a target and controls for industry and time effects.

In the R&D growth regression, the dependent variable is a growth rate and thus purges individual fixed effects. We therefore exclude the acquirer/target dummies from the regression.

$$\text{rdgrowth}_{ij} = \alpha + \sum_{t=1}^6 \beta_t \text{acquirer}_{i,j-t} + \sum_{t=1}^6 \gamma_t \text{target}_{i,j-t} + \eta \text{controls} + \varepsilon_{ij} \quad (5)$$

In both regressions we do not include the merger period (t) to avoid the measurement of consolidation effects.¹³

Even though we construct separate control groups for acquirers and targets, we estimate results jointly in a pooled setting including both targets and acquirers as well as their respective control groups. The results when effects on acquirers and targets are estimated separately in the respective subsamples are very similar to those found in the pooled setting and are reported in the appendix.

Results

Figure 2 charts the mean growth of R&D spending by acquirers and targets around the merger. Prior to the merger (periods -2 and -1) both acquirers and targets exhibit strong R&D growth rates of between 9 and 14 percent. In the year of the merger, the R&D growth of acquirers jumps to almost 24% and then strongly declines in the periods after the acquisition, with a minimum of 2.5% growth 5 years after the merger. Since this spike in R&D growth is a pure bookkeeping phenomenon (consolidation of R&D efforts) and not a causal effect of the merger, we exclude period t from estimation. After this one-period spike, the incentive of acquirers to increase innovative assets seems to diminish.

The R&D growth of merger targets is high in the periods prior to the acquisition, but starts dropping immediately in the period of the merger. From $t - 1$ to $t + 2$, R&D growth declines monotonically from more than 10% to

¹³Since R&D intensity is the ratio of two variables that are both similarly affected by consolidation effects, it would not be strictly necessary to drop t in the R&D intensity regressions. While all results are robust to the inclusion of t , the set of results reported excludes the merger period in order to increase the comparability to the R&D growth regressions.

about 1%. After $t + 2$, R&D growth starts to increase again, without reaching its former level in the observation period. It thus seems, that the acquisition creates a slump in the target's R&D growth profile and that a substantial recovery period is needed to return to the former growth path.

Figure 2: R&D growth of acquirers and targets around the merger

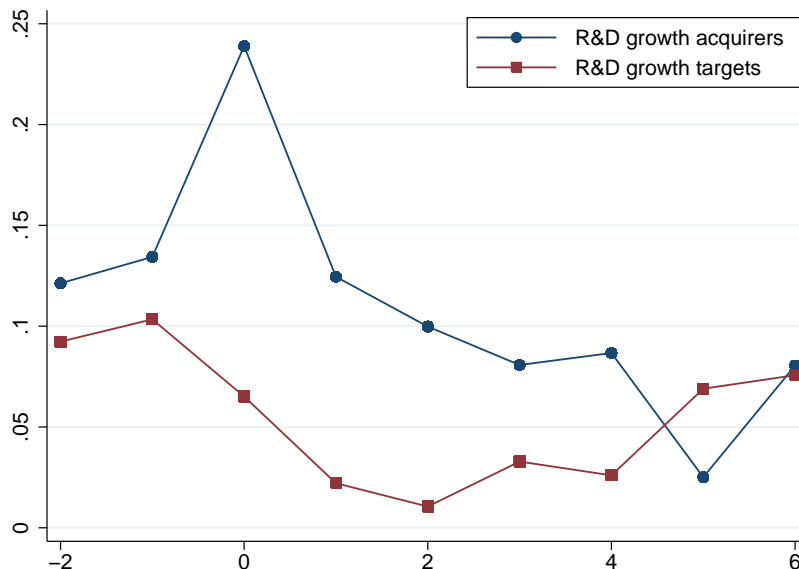
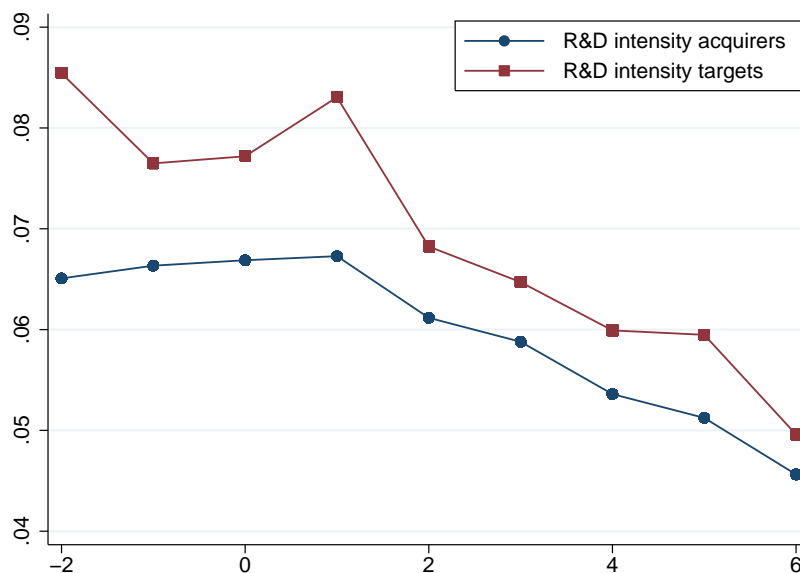


Figure 3 reports the R&D intensity (equal to total R&D spending divided by total sales) of acquirers and targets from two years before until six years after the merger. Prior to the merger, the R&D intensity of acquirers is relatively constant around a high level of 6.5%. Acquirers are, therefore, on average quite R&D-intensive firms. This remains unchanged in the period of the merger and the one after it. From $t + 1$ to $t + 6$ we observe a monotonous decline in the R&D intensity of acquirers, which drops from 6.7% to 4.6%. This effect is not due to consolidation, since R&D and sales are both consolidated. Thus, R&D intensity is reduced by almost a third on average in the five years after an acquisition is made. A similar, but even stronger pattern can be observed in the R&D intensity of merger targets: while starting out at an very high level of about 8%, the graph monotonically decreases to 5%

in the post-merger periods, suggesting a reduction in R&D intensity of more than a third.

From figures 2 and 3 it appears that merger targets are chosen on the basis of being very innovative firms - they exhibit high R&D growth and intensity -, but that their innovative efforts decrease substantially after the acquisition. A similar claim could be made for the buying firms. It thus appears that the incentive to invest in innovation is substantially reduced in post-merger periods.

Figure 3: R&D intensity of acquirers and targets around the merger



While these two figures suggest that certain changes in innovative behaviour occur around a merger, they contain only mean R&D growth and intensity of acquirers and targets, which do not permit inferences as to the significance or causality of the observed phenomena. To achieve this, we run regressions in a difference-in-difference setting (see section) within the relevant control group (see section). The dependent variables are R&D growth and intensity respectively. All regressions are reported in a (acquirers and targets) pooled

setting¹⁴ in the three different samples obtained by nearest-neighbor, caliper and Mahalanobis matching. All specifications include controls for industry and time effects (not reported). The R&D intensity regression includes two further dummies, which control for unobserved differences of acquirers / targets and the control group.¹⁵ The results are reported in tables 3 and 4.

In table 3, the regression results for acquirers in the nearest-neighbor sample show no significant deviation from the control group in all periods except for $t+5$, when acquirers experience significantly lower R&D growth. The same is found in the other two samples, which also show significantly negative effects in $t+3$ and $t+4$ (caliper matching) and $t+2$ through $t+4$ (Mahalanobis matching).

The growth effects on targets are much more clear-cut: in all periods from $t+1$ to $t+4$ and all three samples, merger targets experience lower R&D growth than their peers, significant at the 1% level. The significance of this result drops slightly in $t+5$ and disappears in $t+6$. Thus the R&D growth of merger targets is significantly lower than that of the control group for the five year period after the acquisition has occurred.

The p-values reported at the bottom of the table test the null hypothesis that the sum of all acquirer (or target) timeline-dummy coefficients is not significantly different from zero. Since all of these hypotheses can be rejected at the 1% level, we conclude that the aggregate effect on R&D growth over the six periods following a combination is significantly negative for both acquirers and targets, but more so for targets.

Turning to the regression addressing R&D intensity, we find that the R&D intensity of acquirers is significantly affected by a merger: while the difference to the control group is insignificant in period $t+1$ (and the periods prior to it), all coefficients are significantly negative from periods $t+2$ until $t+6$ in all three samples. The coefficients indicate a cumulative reduction of R&D

¹⁴As mentioned before, all results qualitatively hold when estimating effects on targets and acquirers separately; see appendix.

¹⁵These dummies are not included in the R&D growth regression, since the growth rate specification purges time-constant unobserved fixed effects.

Table 3: R&D growth of acquirers and targets from $t + 1$ to $t + 6$

	Nearest-neighbor matching	Caliper matching	Mahalanobis matching
Acquirer $t+1$	0.011 (0.024)	-0.003 (0.022)	-0.022 (0.025)
Acquirer $t+2$	-0.015 (0.022)	-0.028 (0.024)	-0.048* (0.025)
Acquirer $t+3$	-0.034 (0.025)	-0.047* (0.025)	-0.066** (0.029)
Acquirer $t+4$	-0.028 (0.018)	-0.038** (0.018)	-0.059** (0.022)
Acquirer $t+5$	-0.092*** (0.019)	-0.101*** (0.020)	-0.122*** (0.018)
Acquirer $t+6$	-0.038 (0.054)	-0.049 (0.054)	-0.069 (0.049)
Target $t+1$	-0.088*** (0.013)	-0.103*** (0.014)	-0.124*** (0.017)
Target $t+2$	-0.102*** (0.023)	-0.110*** (0.020)	-0.131*** (0.025)
Target $t+3$	-0.078*** (0.027)	-0.093*** (0.028)	-0.113*** (0.029)
Target $t+4$	-0.086*** (0.029)	-0.100*** (0.025)	-0.121*** (0.031)
Target $t+5$	-0.044* (0.022)	-0.053*** (0.018)	-0.074*** (0.024)
Target $t+6$	-0.039 (0.068)	-0.050 (0.068)	-0.071 (0.070)
Observations	4909	8021	4324
Acquirers	265	263	265
Targets	133	132	132
Σ Acquirer timeline [†]	0.007	0.000	0.000
Σ Target timeline [†]	0.000	0.000	0.000

All regressions include controls for industry and time effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (in parentheses) are robust and allow for clustering on the year-level.

[†] p -Values of the Wald-test with the null-hypothesis, that the sum of timeline coefficients of acquirers (or targets) is not significantly different from zero.

Table 4: R&D intensity of acquirers and targets from $t + 1$ to $t + 6$

	Nearest-neighbor matching	Caliper matching	Mahalanobis matching
Acquirer $t+1$	-0.008 (0.008)	-0.010 (0.006)	-0.005 (0.009)
Acquirer $t+2$	-0.015*** (0.005)	-0.016*** (0.005)	-0.012* (0.006)
Acquirer $t+3$	-0.017*** (0.005)	-0.018*** (0.005)	-0.014** (0.006)
Acquirer $t+4$	-0.023*** (0.006)	-0.024*** (0.006)	-0.019*** (0.007)
Acquirer $t+5$	-0.027*** (0.009)	-0.028*** (0.008)	-0.022** (0.010)
Acquirer $t+6$	-0.035*** (0.011)	-0.035*** (0.010)	-0.028** (0.012)
Target $t+1$	-0.006 (0.013)	-0.009 (0.012)	-0.004 (0.014)
Target $t+2$	-0.023** (0.009)	-0.025** (0.009)	-0.019* (0.010)
Target $t+3$	-0.025* (0.013)	-0.026* (0.013)	-0.022 (0.014)
Target $t+4$	-0.030*** (0.009)	-0.032*** (0.010)	-0.027** (0.011)
Target $t+5$	-0.032*** (0.010)	-0.035*** (0.010)	-0.029** (0.011)
Target $t+6$	-0.044*** (0.013)	-0.044*** (0.012)	-0.037*** (0.013)
Acquirer	0.002 (0.005)	-0.000 (0.005)	0.014 (0.008)
Target	0.019* (0.009)	0.018 (0.011)	0.029** (0.014)
Observations	4909	8021	4324
Acquirers	265	263	265
Targets	133	132	132
Σ Acquirer timeline [†]	0.000	0.000	0.013
Σ Target timeline [†]	0.004	0.002	0.024

All regressions include controls for industry and time effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (in parentheses) are robust and allow for clustering on the year-level.

[†] p -Values of the Wald-test with the null-hypothesis, that the sum of timeline coefficients of acquirers (or targets) is not significantly different from zero.

intensity amounting to 2.8 to 3.5 percentage points in comparison to the control groups in all three samples.

The effects on merger targets are qualitatively similar; although the reduction in R&D intensity seems to be even more pronounced, the standard errors of the coefficients are higher than those of the acquirers, pointing to a wider range of possible outcomes. In all three samples the timeline dummies are negative throughout and indicate significant deviations from the control group from $t + 2$ through $t + 6$ in the nearest-neighbor and caliper samples. Significances are slightly lower in the Mahalanobis sample. The implied reduction in R&D intensity ranges between 3.7 and 4.4 percentage points after six periods.

In the pooled settings reported here, the generally lower level of R&D intensity among acquirers and their control group is offset by significantly positive target dummies in two of the three samples.

Similarly to the R&D growth regressions, we report the p-values of the hypothesis that the sum of all period effects is not significantly different from zero. Most null hypotheses are rejected at the 1% level (those tested in the Mahalanobis sample can only be rejected at the 5% level) suggesting that the R&D intensities of acquirers and targets are significantly negatively affected in the six periods after a merger.

Conclusion

In this paper we have estimated the effect of M&A activity on the growth of R&D spending as well as R&D intensity of the parties involved, using a sample of merger cases that went under the scrutiny of either the EC or the FTC. In doing so, we have explicitly recognized the roles of the firms involved as either buying firms or merger targets and have evaluated the impact on both groups separately, using appropriately constructed control groups.

In terms of merger mechanics, the results suggest, that merger targets are chosen on the basis of being highly innovative firms, as indicated by an average pre-merger R&D intensity of over 8%. The fact that the probability of being a target is negatively related to profitability (as indicated by the propensity

score regression) supports the conjecture, that these firms have not yet been able to reap the profit of their innovative efforts. Acquirers thus seem to cherry-pick firms with attractive technological portfolios, that have not yet been fully commercially exploited. Acquirers themselves, on the other hand, are primarily characterised by being both large and profitable.

We find that the mergers in this sample entail a significant negative effect on the R&D efforts of firms in the subsequent periods. Specifically, looking at mean R&D intensities over time, we find that the R&D intensity of acquirers six years after the acquisition has dropped by almost a third compared to its pre-merger level, while the R&D intensity of merger targets decreases by more than a third. We corroborate these findings in a difference-in-difference setting, where the evolution of merging firms' R&D intensity is contrasted with that of appropriate control groups. While the effects on both groups (acquirers and targets) are unambiguously negative, the effects on acquirers are more significant.

The mergers in this sample also entail detrimental R&D growth effects on both acquirers and targets: while the R&D stock of merger targets accumulates significantly slower than that of the control group in all periods until five years after the merger, this is only true in some periods for acquiring firms. Thus, while both groups of firms experience negative effects in terms of R&D growth as well as R&D intensity, the growth effects are more pronounced on targets, while the intensity effects primarily affect buying firms. This finding can be explained by the fact that average acquirers experience a substantial increase in sales after the merger, whereas they increase their R&D spending only very modestly.

From the point of view of a policy-maker that aims to maximize the well-being of consumers it seems distressing that the average effect of a business combination in a sample consisting of acquisitions that are very diverse in nature, but major in size is unambiguously negative to such an extent. Competition authorities are traditionally reluctant to consider the effects of a merger on innovative activity, since such effects are hard to quantify, particularly from an ex-ante perspective. However, given that the findings of the literature on

the effects of M&A on R&D are predominantly negative, it would seem desirable to find a way to incorporate such considerations into the evaluation of notified mergers.

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Separate regressions for acquirers and targets

Table 5: R&D growth regressions in acquirer/target subsamples

	Nearest-neighbor		Caliper		Mahalanobis	
	Acquirers	Targets	Acquirers	Targets	Acquirers	Targets
Acquirer t+1	0.014		-0.014		-0.020	
Acquirer t+2	-0.012		-0.040		-0.045*	
Acquirer t+3	-0.030		-0.058**		-0.064**	
Acquirer t+4	-0.024		-0.049**		-0.055**	
Acquirer t+5	-0.086***		-0.111***		-0.118***	
Acquirer t+6	-0.032		-0.060		-0.065	
Target t+1		-0.095***		-0.088***		-0.127***
Target t+2		-0.110***		-0.096***		-0.134***
Target t+3		-0.084**		-0.076**		-0.115***
Target t+4		-0.092**		-0.088***		-0.126***
Target t+5		-0.052		-0.044*		-0.082***
Target t+6		-0.048		-0.040		-0.078
Observations	3140	1571	5057	2582	2721	1398
Mergers	265	133	263	132	265	132
Σ timeline [†]	0.006	0.011	0.001	0.002	0.000	0.000

All regressions include controls for industry and time effects. * p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors (in parentheses) are robust and allow for clustering on the year-level.

[†] p-Values of the Wald-test with the null-hypothesis, that the sum of timeline coefficients of acquirers (or targets) is not significantly different from zero.

Table 6: R&D intensity regressions in acquirer/target subsamples

	Nearest-neighbor		Caliper		Mahalanobis	
	Acquirers	Targets	Acquirers	Targets	Acquirers	Targets
Acquirer t+1	-0.007		-0.008		-0.002	
Acquirer t+2	-0.014**		-0.014**		-0.009	
Acquirer t+3	-0.016***		-0.016***		-0.011*	
Acquirer t+4	-0.021***		-0.021***		-0.016**	
Acquirer t+5	-0.025***		-0.025***		-0.019*	
Acquirer t+6	-0.033***		-0.032***		-0.025**	
Acquirer	0.008*		0.002		0.015*	
Target t+1		-0.008		-0.013		-0.008
Target t+2		-0.025***		-0.031***		-0.024**
Target t+3		-0.026*		-0.031**		-0.026*
Target t+4		-0.031***		-0.037***		-0.032***
Target t+5		-0.035***		-0.042***		-0.035***
Target t+6		-0.046***		-0.053***		-0.045***
Target		0.005		0.010		0.023*
Observations	3302	1626	5301	2670	2909	1468
Mergers	265	133	263	132	265	132
Σ timeline [†]	0.001	0.002	0.000	0.000	0.049	0.007

All regressions include controls for industry and time effects. * p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors (in parentheses) are robust and allow for clustering on the year-level.

[†] p-Values of the Wald-test with the null-hypothesis, that the sum of timeline coefficients of acquirers (or targets) is not significantly different from zero.

Abstract

The present dissertation is concerned with the empirical evaluation of competition policy and its implications for competition and efficiency. It comprises three articles, summarized in the following paragraphs:

An Empirical Assessment of the 2004 EU Merger Policy Reform

Based on a database of 326 merger cases scrutinized by the European Commission (EC) between 1990 and 2007, we evaluate the economic impact of the change in European merger legislation in 2004. We first propose a general framework to assess merger policy effectiveness, which is based on standard oligopoly theory and makes use of stock market reactions as an external assessment of the merger and the merger control decision. We then focus on four different dimensions of effectiveness: 1) legal certainty; 2) frequency and determinants of type I and type II errors; 3) rent-reversion achieved by different merger policy tools; and 4) deterrence of anti-competitive mergers. To infer the economic impact of the merger policy reform, we compare the results of our four tests before and after its introduction. Our results suggest that the more economics based approach of the EC resulted in a better identification of problematic cases, however, the fact that the EC essentially stopped blocking mergers does not seem to be well grounded.

Investigating Transatlantic Merger Policy Convergence

We propose a framework to examine tendencies of convergence in the jurisdictional patterns of the American FTC and the European Commission. Based on a sample of 493 merger cases scrutinized by one of these agencies in the 1999 - 2007 period, we calibrate logit models of the probability of intervening in a merger for both jurisdictions and use them to predict the decisions of the respectively other agency. The results point to an increasing harmonization of merger policies and corroborate the theoretical appraisal, that the 2004 reform of EU merger law constituted a step towards the US system.

M&A and R&D: Asymmetric Effects on Acquirers and Targets?

We evaluate the impact of M&A activity on the growth of R&D spending and R&D intensity of 265 acquiring firms and 133 merger targets in the time period ranging from 1990 to 2009. We use a range of matching techniques to construct separate control groups for acquirers and targets and use appropriate difference-in-difference estimation methods to single out the causal effect of mergers on R&D growth and intensity. We find a significant reduction of R&D efforts by both acquirers and targets in the periods after the merger, pointing to a decrease of the incentive to innovate.

Zusammenfassung

Die vorliegende Dissertation beschäftigt sich mit der empirischen Evaluierung rechtlicher Rahmenbedingungen im Wettbewerbsrecht und ist in drei Artikel gegliedert.

Der erste Artikel (gemeinsam mit Tomaso Duso und Klaus Gugler) untersucht die ökonomischen Auswirkungen einer Reform des europäischen Wettbewerbsrechts im Jahr 2004. Anhand einer Stichprobe von 326 Fusionen, welche im Zeitraum von 1990 bis 2007 von der europäischen Kommission evaluiert wurden, wird die Effizienz des Wettbewerbsrechts vor und nach der Reform verglichen, wobei die Effizienz der Jurisdiktion in vier Dimensionen (Rechtssicherheit, Entscheidungsfehler, Reversion antikompetitiver Renten und Abschreckungseffekte) untersucht wird. Dabei zeigt sich, daß der durch die Reform erhöhte Fokus auf ökonomische Analyse in Fusionen nur bedingt Erfolg zeigt.

Der zweite Artikel widmet sich der Frage, ob die Wettbewerbspolitik der Europäischen Kommission und der amerikanischen Federal Trade Commission im Zeitraum von 1999 bis 2007 Tendenzen der Konvergenz aufweisen. Hierzu werden empirische Modelle der beiden Jurisdiktionen kalibriert, die dann verwendet werden um die Entscheidungen der jeweils anderen Behörde zu prognostizieren. Die Ergebnisse weisen darauf hin, daß nach einer Periode steigender Inkongruenz zu Beginn des Jahrtausends die Reform des europäischen Fusionsrechts zu einer weitgehenden Angleichung der beiden Rechtssysteme geführt hat.

Der dritte Artikel untersucht, inwiefern Fusionen die Innovationsbemühungen der beteiligten Firmen beeinflussen. Konkret wird dabei eine Stichprobe fusionierender Firmen mit einer abgestimmten Kontrollgruppe nicht-fusionierender Firmen komplementiert und auf diese Weise der kausale Effekt der Fusion auf Forschungsausgaben und -intensität isoliert. Es zeigt sich, daß fusionierende Firmen ihre Innovationsbemühungen in den Jahren nach einer Fusion deutlich reduzieren.

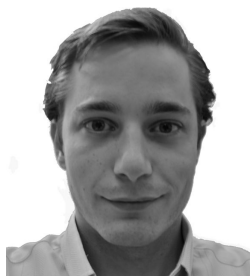
Curriculum Vitae

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'Merger Policy Evaluation: Where Do We Stand?' (with Tomaso Duso and Klaus Gugler) (submitted)

'Investigating Transatlantic Merger Policy Convergence' (Revise and resubmit from *IJIO*)

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'M&A and R&D: Asymmetric Effects on Acquirers and Targets?'

'Spillovers on Merger Rivals'

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